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US ARMY MEDICAL RESEARCH LABORATORY
FORT KNOX, KENTUCKY

SYMPOSIUM
ON
CUTANEOUS SENSITIVITY

11-13 FEBRUARY 1960

GLENN R. HAWKES, EDITOR

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US ARMY MEDICAL RESEARCH LABORATORY

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SYMPOSIUM ON CUTANEOUS SENSITIVITY
11 - 13 February 1960

Capt Glenn R. Hawkes, MSC
Editor

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SYMPOSIUM ON CUTANEOUS SENSITIVITY

Fort Knox, Kentucky
11 - 13 February 1960

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INTRODUCTION

Glenn R. Hawkes, Editor
US Army Medical Research Laboratory
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It was with considerable interest that several months before the 1959 convention of the American Psychological Association, most of the members of the present group received from Professor B. von Haller Gilmer, Carnegie Institute of Technology, an invitation to gather informally during the convention week for a discussion of the "SKINGAME." The group represented by papers herein has contributed greatly to what is known of cutaneous sensory capacities, and shared with Professor Gilmer interest in cutaneous communication possibilities.

The meeting was held at the Netherland Hilton Hotel, Cincinnati, Ohio, on Labor Day, 1959. Those present were Drs. Gilmer, Gregg, Wagoner, Hahn, Hawkes, Kenshalo, and Uttal. After several hours of discussion it was apparent that considerable additional time would be needed if one wished to do more than delineate problem areas. Acting on what was perceived to be the Zeitgeist, the editor therefore proposed that a more formal symposium on the topic be held, and suggested as the site the US Army Medical Research Laboratory, Fort Knox, Kentucky. The Symposium on Cutaneous Sensitivity was held at Fort Knox, Kentucky, 11 - 13 February, 1960.

The purpose of the symposium conceivably is divisible into two main parts: (1) an assessment of progress toward answering some of the basic theoretical questions regarding cutaneous sensory phenomena and mechanisms, and (2) the requirements of an efficient cutaneous communication system.

Consistent with purpose number one, papers were presented on the first day of the symposium by Drs. William R. Uttal, IBM Research Center, Daniel R. Kenshalo, Florida State University, Georg von Békésy, Harvard University, and F. Nowell Jones, University of California, with commentary by our Invited Discussant for this portion, Professor John Paul Nafe, Florida State University. The remainder of the first day was devoted to informal discussion of basic theoretical issues.

The second day was concerned with Topic 2 (above), with papers by Drs. B. von Haller Gilmer, Carnegie Institute of Technology, Lee W. Gregg, Carnegie Institute of Technology, William C. Howell, Ohio State University, John F. Hahn, University of Virginia, and Earl A. Alluisi, Emory University, with commentary by the Invited Discussant for this section, Dean Frank A. Geldard, University of Virginia. A third morning was devoted to concluding remarks and comments by all of the participants.

In addition to the participants, mentioned above, Dr. Carl E. Sherrick, Central Institute for the Deaf, was present at and contributed greatly to all of the discussions. Further, Mr. Rufus L. Grason, Harvard University and Grason-Stadler Co., Inc., demonstrated Professor von Békésy's model of the cochlea and discussed equipment problems.

Present as an observer for the US Army Medical Research and Development Command was Dr. Phillip I. Sperling. Dr. Sperling reported to the group the interest of the Army in supporting meetings of this nature for the purpose of fostering better communication among outstanding scientists.

It is obvious that the myriad details of arranging a symposium of this size cannot all be done by any one person. To the participants, the editor extends his thanks for their cooperation in preparing, presenting, and commenting upon the papers. Appreciation for their very considerable help is also due Colonel H. W. Glascock, Jr., Dr. F. A. Odell, Lt. Colonel E. K. Montague, Dr. Philip I. Sperling, Sp4 Joel S. Warm, Mrs. Janice Duncan, and to those others that space does not permit listing herein.

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Receptive Capacities of the Skin

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In explorations of the skin as another avenue of communication between the external environment and the organism, an understanding of the transducer mechanisms available within the skin and of their method of operation is necessary. Nafe (1934 and 1942) has presented a theory to account for the qualitative differences between tactile and thermal sensations yet which assumes but one type of nerve termination in the skin. Being essentially alike, the nerve terminations have a common adequate stimulus; namely, movement, either in relation to themselves or to the surrounding tissue. These terminals differ primarily in that they terminate in different tissues of the skin. Both tactile and thermal sensations result when tissue is moved, the former from the direct action of a mechanical stimulus on the tissue, the latter, by movement induced in the cutaneous arterioles by the thermal stimulus on the thermally labile smooth muscle of these arterioles. It seems propitious at this time to examine his theory in the light of current evidence.

I. Histology of the Skin. With respect to the receptors of the skin, Rose and Montcastle (1959, p. 390), have reaffirmed the position of most textbook writers that the skin senses are mediated by specialized encapsulated endings. Commenting on the recent publications by the Oxford group (Weddell, Palmer, and Pallie, 1955), in which they maintain that there is but a single type of nerve ending mediating cutaneous sensations, "If a crisis exists in respect to evaluating the morphology of the endings, it is a crisis of abundance and not of scarcity. One hesitates to accept as a solution to the vexing problem of the morphology of the encapsulated endings a declaration that virtually all morphological differences between them are either insignificant or due to artefacts of techniques." Were the Oxford group the only investigators to tender such findings, one might expect such a reception. The controversy of whether or not the encapsulated endings mediate the various cutaneous sensations has a much longer history.

As Figure 1 indicates, the attack on the problem of the histology of cutaneous sensation, three major methods have been employed. The

Investigator	Skin Area		Sensations Tested	Encapsulated Endings Reported
	Glabrous	Hairy		
TYPE I INVESTIGATIONS				
Donaldson (1885)		Forearm	Cold	None
Goldscheider (1886)		Forearm	Cold, Warm, Touch, Pain	None
Häggqvist (1913)		Forearm	Cold, Warm	None
Dallenbach (1927)		Upper Arm	Cold, Warm	None
Pendleton (1928)		Forearm	Cold	None
Woolard (1935)		Thigh	Cold, Pain, Touch	None
Bazett (1941)		Forearm	Cold	None
Weddell (1941)		Forearm	Cold	Krause Endbulbs
Belonoschkin (1933)	Nipple		Cold	Complicated Plexuses Without Capsules
TYPE II INVESTIGATIONS				
von Frey (1895)	Conjunctiva Cornea	Forearm	Cold Cold, Pain, Touch, Warm	Krause Endbulb None Krause Endbulb
Strughold (1925)	Mouth Mucosa		Cold	Krause Endbulb
Strughold & Karbe (1925)	Conjunctiva		Cold	Krause Endbulb
Bazett, et al (1932)	Prepuce		Cold, Warm, Touch	Krause Endbulb Ruffini Cylinders
Sinclair, et al (1952)	Finger		Cold, Warm, Touch	Meissner Corpuscle Merkel's Discs Krause & Ruffini Endings
		Forearm Auricle		None None
TYPE III INVESTIGATIONS				
Gilbert (1929)		Thigh Epigastrium Chest Breast		None None None None
Gilmer (1941)		Back		None
Hagen, et al (1953)		Abdomen Finger Dorsum		None None
Dastur (1955)		''232 Specimens from Hairy Skin''		
Winkilmann (1955)		''Hairy Skin''		None
Woolard (1936)		Thigh		Meissner Corpuscle Merkel's Disc
Gilbert (1929)		Foot Sole Nipple		Meissner Corpuscles Golgi-Mazzoni Capsules
Williams, et al (1929)		Prepuce		Krause Endbulb Ruffini Cylinders Meissner Corpuscles 4 Unnamed types
Cathcart, et al (1948)		Nipple		Krause Endbulb (rare) Unnamed smaller capsules
Gairns (1951 & 55)		Palate Gum		Meissner Corpuscles Krause Endbulb Unnamed Varieties
Gairns (1953)	Tongue		Krause Endbulb Meissner Corpuscles	

Fig. 1.

first involves careful sensory testing and permanent marking of the points on the skin which are sensitive to thermal and/or tactile stimulation. After sensory testing and marking, the tissue is removed, stained, and examined microscopically. This is the most direct approach. The second method involves the general histological examination of the tissue under investigation and relating the kind and number of sensory spots found in that tissue with the types and numbers of end organs found there. The third serves the function of describing the location and distribution of various kinds of encapsulated endings. This involves a general histological study of the skin. The second and third approaches give supportive data at best.

Investigations of the first type have been carried out at least nine times since the first report of Donaldson (1885). With the exception of the study reported by Belonoschkin (1933), these investigations have been carried out on hairy skin. It will be noted that with one exception, the investigators have failed to find encapsulated endings of any sort existing beneath the spots which had previously been demonstrated to be maximally sensitive to various types of stimulation. Investigations of the second type have been conducted primarily on specialized tissue, such as is found in the conjunctiva, the prepuce, and the palmar side of the finger. While there is no direct correlation between a sensory spot and the type of ending beneath it in this type of investigation, in general, these investigators conclude that the number and type of encapsulated endings correspond very well with the number and type of sensory spots reported in that tissue. The list of investigations of the third type included here is by no means an exhaustive list of all of the histological investigations of skin. The inclusion of these authors is on the basis of their interest in cutaneous sensation. It will be noted that, with one exception, when specimens of skin were removed from the hairy regions of the body, encapsulated endings were not found; whereas, when the specimens of skin were obtained from the non-hairy portions of the body, encapsulated endings were quite prevalent. In this entire list of investigations there are three exceptions to the general conclusion that encapsulated endings exist in glabrous skin, and do not occur in hairy skin. These are Weddell, in the first type of investigation; von Frey, in the second type of investigation; and, Woolard, in the third type of investigation. Weddell has since adopted the view that only free nerve endings occur in hairy skin. Woolard, a contemporary of Weddell, and director of the Oxford laboratory when Weddell worked there, has since died, as has von Frey. Weddell, et al (1955) now contends that the structures which he and Woolard reported as being encapsulated endings were either unusual sections of hair follicles, or artefacts which resulted

from rough handling of the specimens during the fixing process. This appears to be reasonable in view of the results obtained by the other investigators.

The conclusion is inescapable that encapsulated endings of one sort or another do occur in the conjunctiva of the eye, the mucous membrane of the mouth, the palmar surfaces of the fingers, and the palms of the hand, the soles of the feet, and the genitalia. On the other hand, in hairy skin, which comprises approximately 90 per cent of the body surface, nerves terminate as axioplasmic filaments. The manner of termination of afferent nerves serving hairy skin has been investigated by Weddell, Pallie, and Palmer (1954), and the work of other investigators has been reviewed by Weddell, et al (1955). In general, afferent nerves terminate as axioplasmic filaments in relation to three types of structures found in the skin. These are (1) as bare axioplasmic filaments terminating in and among the epidermal tissue, among the cells of the stratum granulosum of the epidermis and among the cells of the dermis; (2) some of the largest fibers found serving the skin end in relation to hair follicles, again as axioplasmic filaments entwined about the hair shaft; and, (3) other fibers, somewhat smaller than those ending freely or in relation to hair follicles, terminating as axioplasmic filaments in relation to the smooth muscle elements of the arterioles.

If hairy skin is innervated as described, what has happened to its apparent punctate sensitivity which made the specialized encapsulated receptor theory so attractive? When threshold and near threshold stimuli are used the skin is undoubtedly maximally sensitive at only a few isolated spots as described by Blix (1884), and Goldscheider (1884). As the intensity of the tactile (Guilford and Lovewell, 1936), or thermal (Jenkins, 1940), stimuli is increased, the number of sensitive spots increases markedly. It appears that the skin is continuously sensitive to tactile and thermal stimuli, but exhibits peaks and valleys of sensitivity.

II. Cutaneous Sensation. Since it is no longer possible to hypothesize encapsulated end organs which could be sensitive to various types of energy applied to the skin, we must look for another organizing principle. Nafe (1942) has suggested such a principle in terms of the stimulus which is adequate to excite activity in the nerves supplying the skin. The terminals of these fibers appear morphologically alike, although it cannot be maintained that they are identical (Weiss, 1949; and Sperry, 1950). On the other hand, there seems to be little reason to assume that the differences among nerve fibers are sufficiently great to account for the wide variety of sensory experiences derived from their stimulation.

Since there is no reason, at present, to assume that they are different, it would be expected that these cutaneous fibers would share a common adequate stimulus, regardless of the tissue in which they terminate. Evidence has been presented (Nafe and Wagoner, 1941; Nafe and Wagoner, 1941a; Nafe and Kenshalo, 1958), which suggests that movement is this common adequate stimulus. Hence, tactile sensations are aroused by and during movement of tissue. Thermal sensations are also aroused by tissue movement; however, the tissue in this instance is the smooth muscle composing the walls of the cutaneous arterioles.

A. The tactile sense. Two components exist in the application of any mechanical stimulus to the skin. These are the static component (or displacement), and a dynamic component (or velocity and acceleration). The latter, acceleration, has not been well investigated, although other work (Hubbard, 1958) seems to indicate that it is incidental.

The earlier theories concerning the nature of the adequate stimulus for the tactile sense considered the static component of the mechanical stimulus to be the relevant aspect of that stimulus. Thus, we have the pressure theory (Weber, 1946), the gradient theory (Meissner, 1859), and the tension theory (von Frey and Kiesow, 1899). Von Frey and Kiesow formulated their theory on the basis of stimuli involving very small areas; that is, stimuli with radii of less than one tenth of a millimeter. They found that tension (weight per unit radius), was related to the threshold of sensation for these very small stimuli. On the other hand, for larger stimuli, greater than four-tenths of a millimeter radius, they were able to relate only the rate of application of the stimulus with the threshold of sensation. Apparently, von Frey and Kiesow were so convinced of the static component of the mechanical stimulus that they ignored the rate factor involved in the stimuli of larger radii. Nafe and Wagoner (1941; 1941a) were the first to record the action of a mechanical stimulus in producing a tactile sensation.

Weights, of various sizes, were lowered by means of "L" at the bottom of Figure 2, onto the stimulator "N", and allowed to sink into tissue. The action of the stimulator was recorded by stylus "D", to the left of the diagram, on smoked paper through the pulley system at the top of the diagram. Magnifications of 40 - 1 were obtained by this arrangement. The areas of stimulation were the web of skin between the thumb and forefinger, and the skin just above the knee cap. Among the variables of weight, area, time to cessation of the sensation, and the rate of stimulus movement at the cessation of sensation, they were able to establish a relationship between weight and the rate of stimulator

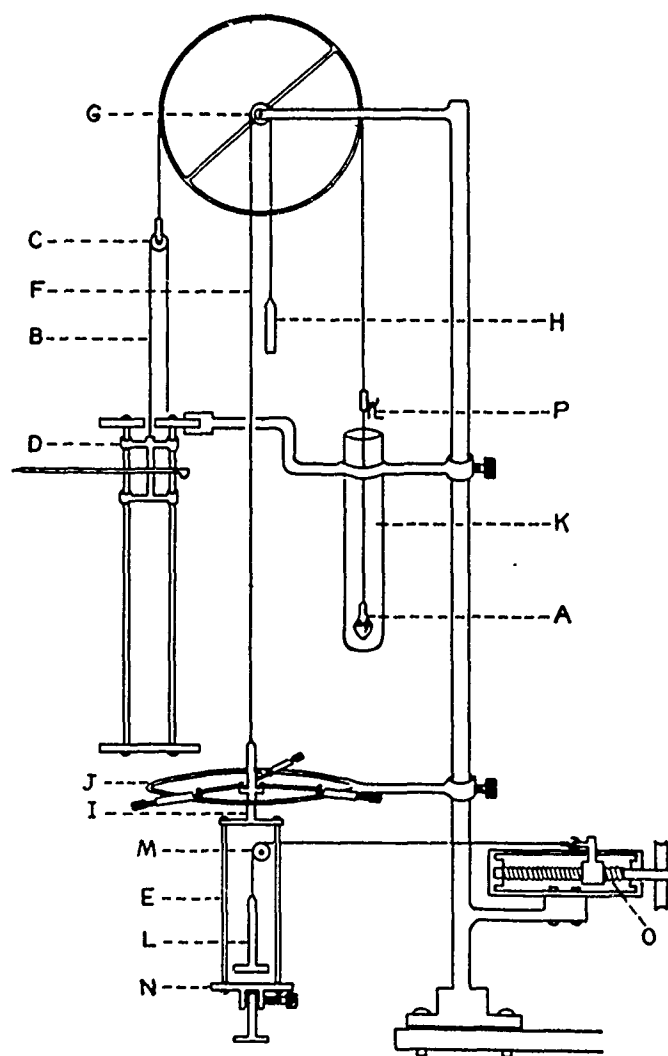


Fig. 2. Diagram of the stimulating and recording apparatus.

movement at the time of cessation of the tactile sensation. Since area varied independently of the rate of movement at the time of cessation of sensation, any aspect of the stimulus, such as pressure and tension which involves this variable, must be rejected as of no importance in producing and maintaining a tactile sensation. Furthermore, they found that the depth of deformation of the tissue was a function of the pressure and also must be rejected. In other words, they were not able to relate any static aspects of the stimulus to the point of cessation of the sensation. As a result they concluded that the dynamic component was the relevant aspect of the stimulus; further, that cessation of the tactile sensation was a result of failure of the stimulus to stimulate, rather than failure of the receptor to respond to the stimulus.

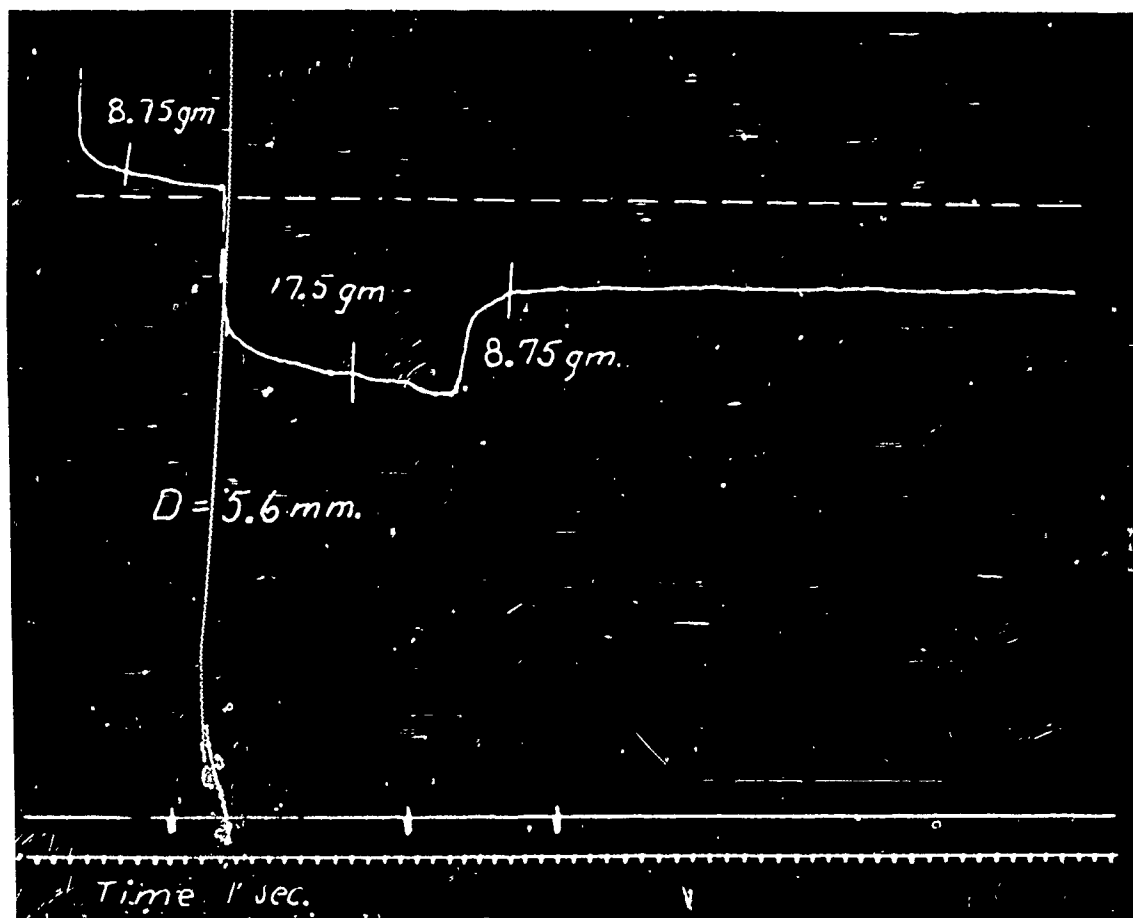


Fig. 3. Showing similarity in effect for sensation and for adaptation whether stimulus is increased or decreased. The curve is reversed but complete adaptation occurs at approximately the same rate of movement in either case. The effect for sensation of removing a stimulus often is reversed.

In order to test this notion, a weight of 17.5 grams was lowered onto the skin in 8.75 gram increments (see Fig. 3). The subject reported a tactile sensation with application of each increment as indicated by the vertical line on the record of the stimulus. Furthermore, when the second 8.75 gram weight was removed the subject again felt a tactile sensation which disappeared at the third vertical line on the stimulus trace. They contend that had the receptors failed rather than the stimulus, the subject would not have felt the application of the second weight, nor would he have felt its removal.

In order to locate this effect in the peripheral receptor mechanism rather than in the central nervous system, Nafe and Kenshalo (1958) repeated Nafe and Wagoner's experiments using a slightly modified version of the Nafe and Wagoner apparatus.

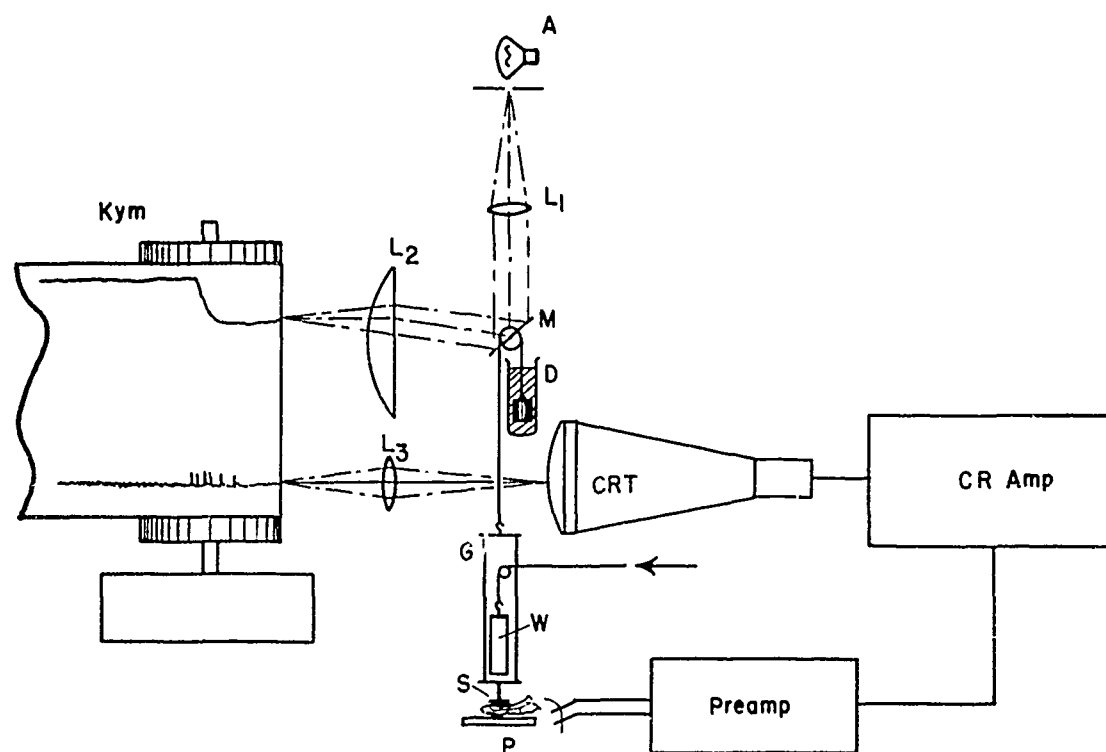


Fig. 4.

In place of the human observer, an animal, usually a rat, was placed on the preparation table at "P", shown in Figure 4. Instead of a verbal report by the subject, the neural discharge from the tissue stimulated was recorded through suitable electronic devices. Simultaneous recordings of the stimulus action and the neural discharge were made on photographic paper as shown in the diagram. In most of these preparations the tongue of the rat was stimulated and recordings were made from the lingual branch of the fifth cranial nerve. Some preparations were made of the shaved leg of the rat recording from the femoral nerve, and in still others, the skin of frog was used.

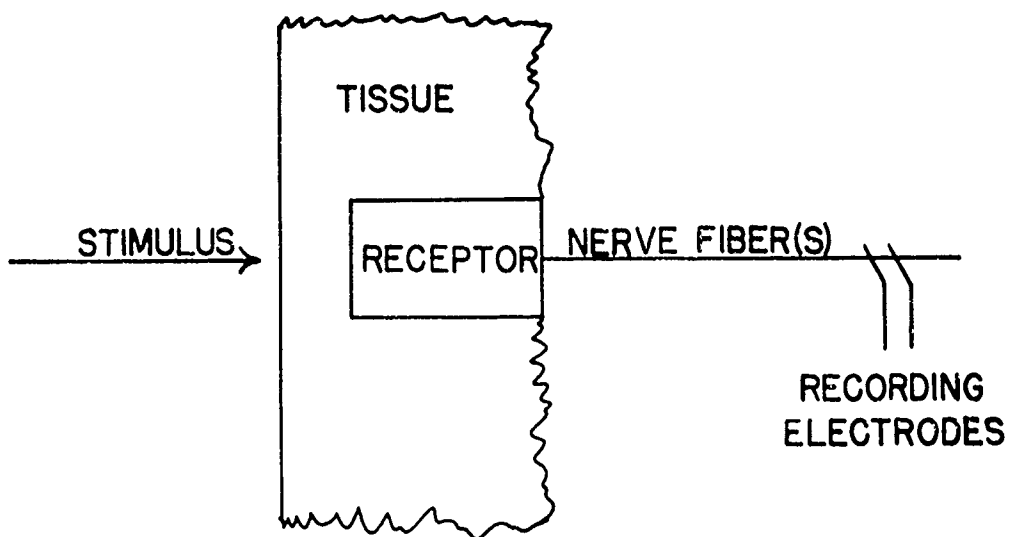


Fig. 5.

In these preparations, shown in Figure 5, we are interested only in the relationship of the stimulus parameter and the neural response produced by them. We are not concerned with intervening steps between the application of the stimulus and the production of nerve impulses.

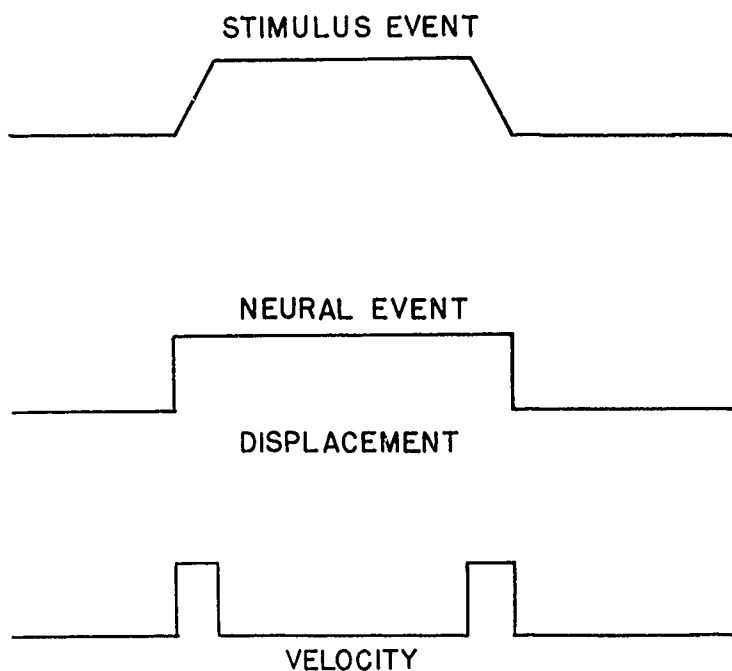
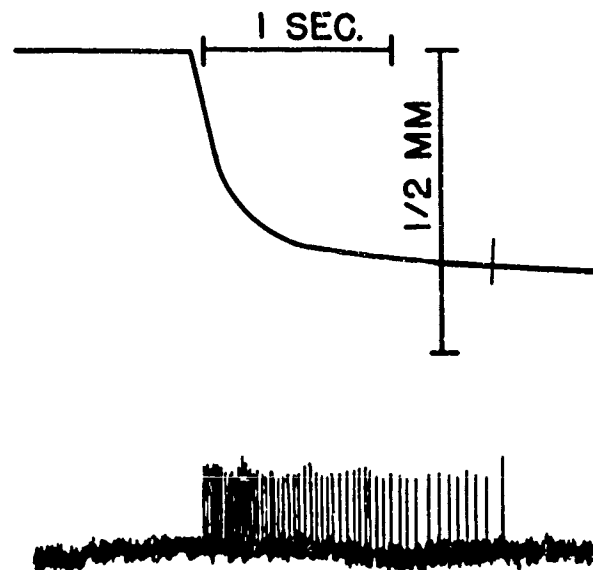


Fig. 6.

Figure 6 indicates the sorts of records which we might expect if the receptor responds either to the dynamic component or to the static component of the mechanical stimulus.



TONGUE-LINGUAL-RAT
1 3-23-54
8.75 GM.

Fig. 7.

It is fairly obvious from Figure 7, shown above, that the stimulus is still sinking into the tissue at the point at which the neural discharge ceased. We have tried this with other weights up to 70 grams applied over the same 12.5 sq. mm area and the record is much the same. Area is not a factor since these are single fiber preparations. Records such as these establish the effect of the cessation of sensation associated with a minimal rate of movement as being a peripheral phenomena. However, the question still remains as to whether the cessation of discharge is a function of the stimulus alone, or if it is in part due to adaptation of the receptor.

To test this, the stimulus was driven into the tissue hydraulically in two steps, the second of which was less than the first. The prediction was that if the cessation of discharge is associated with an adaptation phenomena rather than a stimulus parameter the neural discharge associated with the second step would be less than that of the first. On the other hand, if rate of movement is the determining condition, since the rate at which the stimulator is driven into the skin is the same for both steps, the discharge to the second should be equal to or greater than that from the first step. As can be seen from Figure 8, page 11, the second hypothesis is confirmed. The longer discharge associated

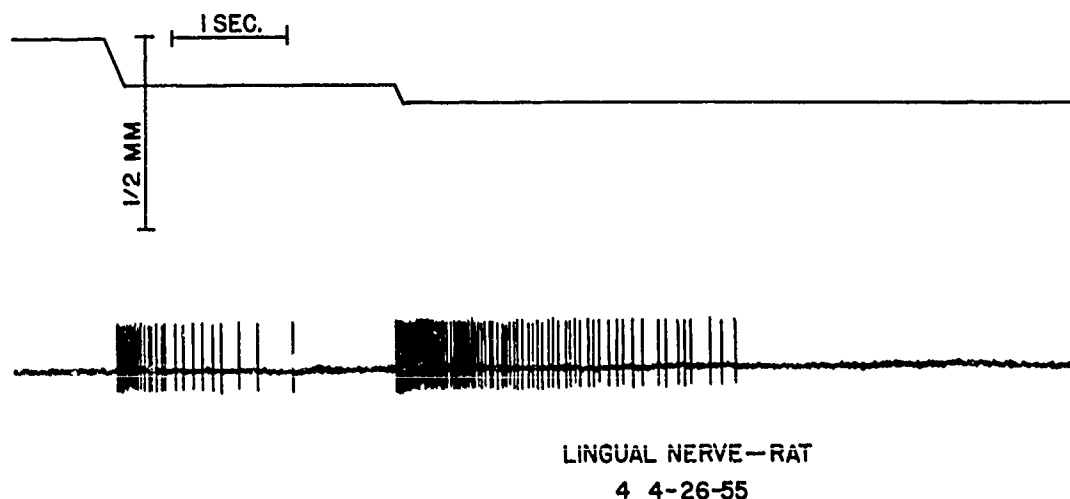


Fig. 8.

with the second step is explained by greater tissue compression following the application of the first step, then prior to the application of the first step.

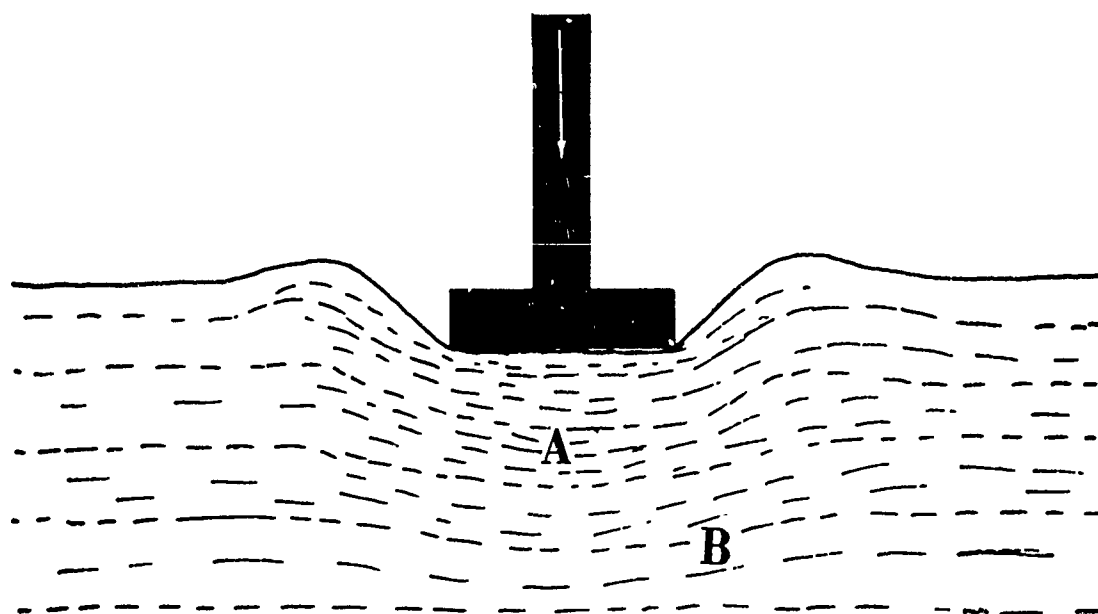
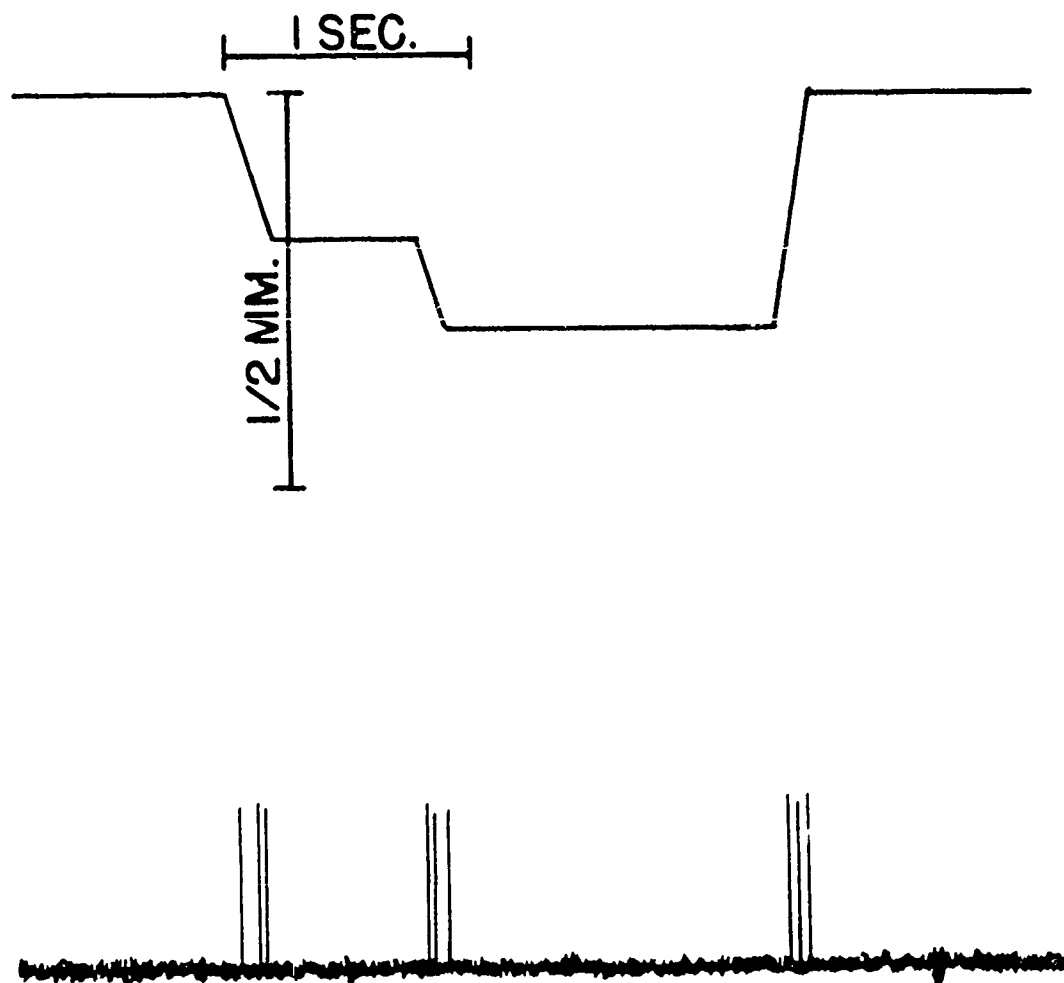


Fig. 9.

Thus, if it is assumed that the termination of the fiber from which the record was made was at "A", in Figure 9, it is immediately apparent that movement at "A" will be greater when the tissue is already under mechanical stress than prior to the initial

compression of the tissue. In order to test this explanation a hard tissue, in comparison to that of the rat's tongue was indicated.



DOR. CUT. FROG
3 8-19-55

Fig. 10.

Frog skin is suited for such an investigation. It is thin and hard, making it better able to follow the action of the stimulus throughout its extent. As can be seen above in Figure 10, the neural discharge occurred only during the time that the tissue was being moved. The second interesting aspect is that when the stimulator was removed from the frog's back a third series of neural impulses occurred.

Since Nafe and Wagoner used the skin of the web between the thumb and index finger and that just above the knee, and Nafe and Kenshalo used the tongue, the hind leg of rat, and frog skin, it must be presumed that in the fibers responding to mechanical stimulation, regardless of how they terminate, their adequate stimulus is movement of the tissue. Since the knee is hairy and Adrian (1930) has demonstrated only the movement of hair is associated with the discharge of fibers supplying the follicle in cat, it can further be assumed that nerves ending in relation to the hair follicle as well as ending freely among the cells of the epidermis respond in a similar way to the same aspect of the stimulus--movement.

The work by Gray and Malcolm (1950), Gray and Matthews (1951), and Gray and Sato (1953), have demonstrated that the Pacinian corpuscle is likewise responsive to only the dynamic aspect of the stimulus--movement.

B. The thermal sense. Two types of receptor mechanisms have been proposed for mediating the thermal sensation. The first assumes that cutaneous nerves are differentially sensitive, some responding to mechanical stimuli while others respond only to a change in thermal energy. This mechanism will require at least two and possibly three different types of nerve fibers. Several mechanisms have been suggested to account for this differential sensitivity. Bishop (1946) has related it to characteristics associated with the size of the fiber involved. Others have suggested that differential sensitivity to chemical changes produced by changes in thermal energy may account for the differential sensitivity of cutaneous terminals. This would require one set of terminals to be responsive to mechanical stimulation, a second set responsive to chemical changes, part of which respond to the change produced by cooling, while the others respond only to the chemical change produced by warming.

The other proposal assumes that endings of the skin respond to a common adequate stimulus, movement. Thermal reception then would be by means of tissue mediated movement resulting from thermal stimulation. It is well known that smooth muscle is thermally responsive, responding in general by contraction to cooling and relaxation to warming. Such movement induced in smooth muscle of the cutaneous arterioles would produce impulses in the nerve fibers terminating among these smooth muscle elements.

Before examining the proposals for thermal receptors the facts concerning thermal sensation should be examined. These impose rather stringent requirements upon any theory of the nature of thermal reception. It should be borne in mind that the thermal sense is different from other

senses in that in the other senses absence of sensation is associated with near zero energy levels. Zero thermal energy is at absolute zero, approximately $-273^{\circ}\text{C}.$, yet psychological zero, or zero thermal sensation, is approximately $33^{\circ}\text{C}.$ To further complicate matters, psychological zero is known to shift markedly within a space of a few minutes by as much as one-half degree (DuBois, 1941), yet during these shifts the subject is unaware of any thermal sensation. Considering that psychological zero is normally between $31^{\circ}\text{C}.$ to $33^{\circ}\text{C}.$ it may be shifted to as low as $29^{\circ}\text{C}.$ or as high as $36^{\circ}\text{C}.$ before the subject experiences a thermal sensation, provided the change is sufficiently slow (Hensel, 1950). The shifting zero point accounts for the frequent observation that the thermal reception system in man is not a good absolute thermometer. While it may be extremely sensitive to small changes exerted in short intervals of time it is amazingly insensitive to large changes produced over long periods of time. Second, the thermal threshold is a function of the skin temperature at the time that the threshold measure is taken (Hensel, 1950; Kenshalo, Nafe and Dawson, 1960). Third, the thermal threshold is a function of the rate of change of the threshold stimulus (Hensel, 1950). While these are not all of the limiting characteristics for which a theory of thermal reception must account, they constitute the major considerations.

In addition to being able to specify what the theory must account for, it can also be stated to some extent, the manner in which theories may not account for thermal sensation. In general, theories which rely on a spatial gradient of temperature, approximately perpendicular to the skin surface, must be rejected. Ebbecke (1917), and Bazett and McGlone (1932) have suggested spatial gradient theories. The suggestion of Bernhard and Granit (1946), and von Euler (1947), that nerve itself may act as a thermal receptor, is also based on regional cooling of the nerve. Lele, Weddell, and Williams (1954) have extended this notion and suggested a "bolometer theory" which depends on warming and cooling stem axons in relation to their terminals. Rejection of spatial gradients as a means of stimulating thermal receptors requires some justification. Hensel and Zotterman (1951), using electrophysiological techniques and locating the termination of a fiber responding to cold close to one side of cat's tongue, find it is immaterial as to which side of the tongue the cooling is applied with respect to the way in which a receptor performs, although the thermal gradient across the receptor is reversed. The older work of Bazett, McGlone, Williams and Lufkin (1932) on the prepuce, likewise, demonstrates the ineffectiveness of the direction of the thermal gradient as a means of stimulating warm receptors. Vendrik and Vos (1958) have recently reported on the use of very long electromagnetic

radiations (approximately 10 cm.) in warming the skin. Such radiations penetrate the skin readily and they are able to demonstrate, in warming the skin with these wave lengths, that the deeper tissues were warmed as rapidly as the surface tissues, abolishing a gradient altogether. The threshold for warmth sensations, however, was found to be about the same as that found by employing more conventional means of stimulation.

Considering the first alternative, that of fibers of different responsiveness, several investigations have purported to demonstrate such differences. Hensel and Zotterman (1951a) have shown that, in general, fibers which respond to mechanical stimuli do not respond well to thermal stimuli and vice versa. Their findings indicate that mechanically sensitive nerves are of large diameter (larger than 8 mp.), whereas, those responding to thermal stimuli are smaller (1.5 mp. to 5 mp.). While they interpret their findings as indicating that nerves are different in their responsiveness it does not necessarily follow that the nerves themselves differ. Such differences could occur as a result of tissue differences, in which they terminate.

Bazett and McGlone (1932a) have suggested that thermal sensations might well be the result of blood acidity. Dodt (1956) has recently demonstrated a differential effect of CO₂ tension on the response of nerves stimulated by warm and cool stimuli. Jenkins (1941) in propounding his concentration theory has also suggested a chemical intermediary not unlike that proposed by Hecht (1934) in the photochemical theory of brightness vision. Bare nerve endings, according to Jenkins, would discriminate between a catabolic and an anabolic phase of some chemical reaction brought about by a change in temperature. Presumably bare nerve endings responding to mechanical stimuli would be insensitive to these changes.

Reminiscent of the anabolic--catabolic hypothesis is the suggestion of Hensel and Zotterman (1951a) on the mechanism of the stimulation of cold receptors. Because of the rather complex shifts in the frequencies of impulses which they report in fibers responding to a lowering of temperature they suggest that in stimulation of the cold fiber there are two temperature dependent processes, probably chemical (Zotterman, 1959). The impulse frequency resulting from cold stimulation is a function of the difference between these two processes. Although they do not speculate on the response to warming the tissue one must presume at least one, and probably two, additional processes are necessary. Dodt (1953) has shown that there are some differences in the response of nerve to warming and cooling as a function of their diameter. The

differences do not appear great enough, however, to aid in understanding Hensel and Zotterman's suggestion of a chemical process controlling the responsiveness of certain fibers to thermal stimuli.

It is difficult to conceive of a chemical process which is in equilibrium at the mid-range of the thermal energy continuum, which may have its thermal equilibrium point shifted by a degree or more if done sufficiently slowly yet will become unstable when changed by as little as 0.003°C . (Hardy and Oppel, 1936). Relying on the mechanisms of the central nervous system even for part of the explanation is considered to be unsatisfactory until all peripheral possibilities have been exhausted.

An alternative, involving fewer assumptions, is that nerve fibers and their terminations are largely alike, and further that differentiation necessary to account for thermal and mechanical responsiveness occurs as a direct result of the tissue in which the fibers terminate. It has been demonstrated that nerve terminals, similar to those terminating among the cells of the epidermis, end among the smooth muscle fibers of the cutaneous arterioles (Woolard, 1926; Weddell, *et al*, 1954). Smooth muscle is known generally to be thermally responsive, relaxing upon warming and constricting when cooled. Furthermore, Nafe and Wagoner (1941; 1941a) have demonstrated that movement of the nerve terminal either upon itself or in relation to its surrounding tissue best fits the facts concerning mechanical reception.

Goldscheider (1886) probably was the first to suggest the relationship between the cutaneous vascular system and thermal sensation. He noted in his excision experiment the "striking and immediate proximity" of blood vessels to cold spots which he had previously mapped. Elaborating on this, Nafe (1934) pointed out the striking relationship between changes in the smooth muscle response to temperature and the subjective reports of stimulation by the same temperature.

Figure 11, page 17, shows that from physiological zero up through approximately 45°C . the subject experiences warmth. There is a general relaxation of smooth muscle within this range. At 45°C . up to 50°C . the subject experiences heat and in smooth muscle there are constricting elements in the generally dilating system. Above 52°C . pain is sensed and spastic constriction of the vessels occurs, shortly followed by death of the muscle element. From physiological zero to 12°C . the experience is cold, and smooth muscle within this range of temperature contracts. Below 12°C . the subject experiences a sensation which is frequently confused with heat, although, from other cues

<u>EXPERIENCE</u>	<u>TEMPERATURE</u> (C°)	<u>ACTION OF SMOOTH</u> <u>MUSCLE</u>
Pain	Above 52	Spastic contraction
Heat	45 - 52	Constricting elements in dilating muscle
Warm	34 - 44	Relaxation
Zero	33	Physiological zero
Cold	13 - 32	Contraction
"Cold heat"	3 - 12	Muscle elements showing severe constriction in general contraction
Pain	Below 3	Spastic contraction

Fig. 11.

it soon can be identified as cold. Smooth muscle elements below 12° C. show severe constriction in a generally constricting system. Below 3° C. pain occurs on the experience side and there is a spastic constriction of the smooth muscle elements which ends with their death.

The vascular theory can account for the requirements of a thermal theory, as presented earlier, quite readily. First, the shifting psychological zero with no attendant thermal sensation is accounted for in terms of the constriction or dilation of the cutaneous arterioles occurring slowly enough to be below the threshold rate of movement necessary to excite the nerve terminals in the vessel walls. On the other hand thermal sensations occurring with no environmental temperature change; e. g., those attending a blush or scratching a finger across a black board, result from reflexly aroused vascular dilation or constriction. Second, a minimal rate of temperature change is necessary in order for a thermal sensation to occur. Certainly, the more rapid the movement of the muscle in the vessel wall, the more intense the discharge of the fibers ending there and hence the more intense the thermal sensation. Third, it would be expected that the degree of relaxation would affect the size of the thermal threshold. This would account for the observations of Hensel (1950) and Kenshalo, *et al* (1960) that the size of the thermal threshold is related to the skin temperature.

In addition to fitting these requirements the theory also handles other facts which for other theories have been somewhat perplexing. In the first place, it is not necessary to postulate different types of free nerve endings, each responsive only to one type of energy. Second, it is not necessary to conceive of two different systems--one for warmth and one for cool. Two possible explanations exist. One depends upon neural patterns while the other depends upon different fibers being stimulated, depending upon the way in which they terminate among the smooth muscle elements. In the first instance neural patterns aroused by constriction could quite conceivably differ from those aroused by relaxation. Patterns in other systems result in different sensations. For example, the peristaltic waves in the stomach passing from the cardiac to the pyloric valves are associated with hunger pangs, while those same contractions passing from the pyloric to the cardiac valves result in the entirely different sensation of nausea. The direction of travel is simply a matter of timing. In addition to central factors involved in thirst the patterns aroused in the membranes of the throat when they are dry as compared to when they are wet contribute markedly to this sensation. The urge to micturate and to defecate are likewise results of patterning of neural discharges.

On the other hand, Hensel and Zotterman (1951), using electrophysiological methods on the cat's tongue, have described two apparently different sets of nerve fibers, one responding to warm and the other to cool stimuli. In striate muscle we have such systems; one responding when the muscle is contracted and the other when it is stretched. In the first instance the receptor elements are connected in series with the muscle fibers, whereas, in the latter case they are connected in parallel with the muscle fiber. Whether the same thing exists in smooth muscle has not been investigated, although Fischer (1944) has described some afferent terminals in smooth muscle as "spray-like", whereas, others are "spindle-like."

Hensel and Zotterman (1951a) have described fibers responding to thermal stimulation as smaller (1.5 - 5 μ .) than those responding to tactile stimulation (8 μ . and larger). Investigations by Weddell and Pallie (1954) have shown that many of the fibers innervating the blood vessels are of dorsal root origin. In addition, these fibers ending in blood vessels are some of the smallest found in the skin, being, for the most part, less than 3 μ . in diameter. The fibers ending in vessels, then, correspond with the fiber spectrum which Hensel and Zotterman have described as responsible for thermal sensitivity.

One of the most frequently cited reasons for considering warmth and cold as mediated by different receptor mechanisms is the difference in reaction times of an observer to warm and cool stimuli, the latter being shorter. Bazett, et al, (1932) and Hensel and Zotterman (1951b) have made much of this difference in latency, using it to calculate the depth of the receptors. These calculations are based on the assumption that the latency of response of the receptors once they have been warmed or cooled by a constant amount are the same. An alternate explanation, in terms of the vascular theory, is equally possible. Constriction is an active process requiring little time to accomplish. On the other hand, dilation is a passive response and should require a much longer time to reach a threshold rate of movement.

Paradoxical cold provides little difficulty for the vascular theory. Paradoxical cold is a term ascribed to the situation in which a cold sensitive area is stimulated with a temperature of 45° C. or above and a cold sensation results. It will be recalled from Figure 11 that at 45° C. smooth muscle has constricting elements within a generally dilating system. It might be expected that a spot normally responding to a cool stimulus would also produce a cool sensation when stimulated by a temperature of above 45° C. It is interesting to note that Zotterman (1953) reports that fibers which normally respond to cooling, after a period of no response to temperature above 35° C. suddenly start to respond when stimulated by temperatures of 45° C. and higher.

Summary

An alternate view has been presented which is believed to explain the facts concerning skin sensitivity as well if not better than that supported by a number of other investigators working on sensory processes. It assumes that nerve terminals do not differ markedly, in themselves, in their response to various types of stimulus energy. Variations in response of nerve to various types of stimulation can be explained as readily by assuming these are the result of the tissue variations in which the terminals end as by assuming fibers of different sensitivity.

It has been demonstrated that movement of the terminal in relation to its surrounding tissue is the adequate stimulus for those fibers serving the tactile sense. Tissue movement is assumed to be the adequate stimulus for those serving the thermal sense as well. Smooth muscle has been shown to alter its response to thermal changes at temperatures which correspond, roughly, with changes in the thermal sensations. Sensations of warmth and cool may result simply from variations in the

pattern of neural activity resulting from dilation or contraction of smooth muscle in the cutaneous arterioles or as a result of the way in which the fibers terminate in relation to the smooth muscle elements.

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The Neural Coding of Somesthetic Sensation: A Psychophysical-Neurophysiological Comparison¹

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One of my favorite comparisons of neurophysiological and psychophysical data is that made by Adrian² displaying the time course of brightness as reported by one of us, a fellow human being, and also the time course of the firing rate in the optic nerve of one of them--an eel. Adrian himself refers to this as a comparison between "chalk and cheese", thus expressing his dissatisfaction with such observations. Indeed, the comparison itself is strained for the time scales are drastically different.

The ideal experiment, to make a comparison of this sort, would consist of psychophysical and neurophysiological research on the same subject, but two difficulties have long plagued this approach. First, we face the tedious and difficult periods required to train animals to make discriminative reactions analogous to the psychophysical experiment in man. Even then the typical experiment is limited to a few points of the full continuum of the dimension we would like to study because of the limited response repertoire of the animal. Furthermore, once having trained an animal to such high levels, one is loathe to sacrifice him to the irreversible neurological dissection. Second, it is only under the most extreme conditions of disability that any neurosurgeon feels justified in macroelectrode probing in a human. The ability of man to contribute psychophysical responses can best be appreciated by the abundance of the current literature. Thus, the suggested ideal experiment has been handicapped for these two reasons.

It is my feeling that the neurophysiological recording limitations when we work with man are only technical problems which are being successfully challenged. On the other hand the psychophysical limitations encountered in animal experiments are fundamental bounds defined by the specimen itself.

It was for this reason I was very excited when Burt Rosner of Yale called my attention to Dawson's researches in recording compound action

¹Although it is difficult to acknowledge all of the people who directly or indirectly contributed to this paper, I would like to especially point out my debt to B. S. Rosner of Yale University and to Miss Louella Cook of our laboratory who has contributed so much to our research program.

²Referred to by Granit in his 1955 Monograph, Receptors and Sensory Perception. New Haven: Yale University Press, 1955.

potentials from intact human peripheral nerves. Herein lay the opportunity to make some really direct comparisons. No longer would we be comparing chalk and cheese, but, if I may, at least two different kinds of cheeses. Previously, Armington's group at Walter Reed had exploited another surface bioelectric phenomena, the electroretinogram, to answer some important questions on the visual process. Although his work was carefully executed it had dissatisfied me for the electroretinogram appears to be a reflection of generator potentials rather than the actual pulse coded conducted action potential. The somesthetic system of a psychophysically responding man with its long peripheral nerves and the surface electrode was, indeed, the embodiment of the ideal experiment.

I would like to discuss in the remainder of this paper our researches in comparing neural and psychophysical responses in man which have led to a clarification, of the problem of intensity coding. Then I would like to discuss some recent developments which have restimulated discussion of somesthetic quality coding.

About the same time that I was introduced to Dawson's technique, I also encountered S. S. Steven's work on the estimates of sensory magnitude. This technique for directly evaluating sensory intensities seemed ideally suited for our purposes.

So armed with these two experimental tools I set out to make the direct comparisons between neural and psychophysical responses which had intrigued me so much. An elaborate electronic system was developed for providing pulse electrical stimuli to surface electrodes.

Electrical pulses were chosen as stimuli for a number of reasons. Far more than longer lasting direct or alternating current, they approximate the time dimensions of the nerve impulse itself and, as we shall see, time is a very important variable in our experiment. Pulses are completely calibrated by the two dimensions of duration and amplitude and within the duration of the pulse the stimulus level can be held constant, unlike the alternating current stimulus which varies continuously during its own time. Even more important was the fact that a considerable body of knowledge based upon pulse stimulation of nerves existed. For example we could use the measures of chronaxie or refractory period in a meaningful way if square stimuli pulses were used. Figure 1 shows the system which was ultimately developed. A pulse generator shapes and forms the pulses. Its output is fed into a power amplifier which feeds a stimulus isolation unit. This isolator is necessary not only to reduce electrical artifacts but also to act as a safety device. As the experimental program progressed we added a constant current device which provided a pulse of current rather than the constant voltage pulse from the isolator to the stimulus electrodes. The problems of electrode localization and control have been discussed earlier.³

³Uttal, W. R., "A Comparison of Neural and Psychophysical Responses in the Somesthetic System", J. Comp. Physiol. Psych., 1959, 52, 485-490.

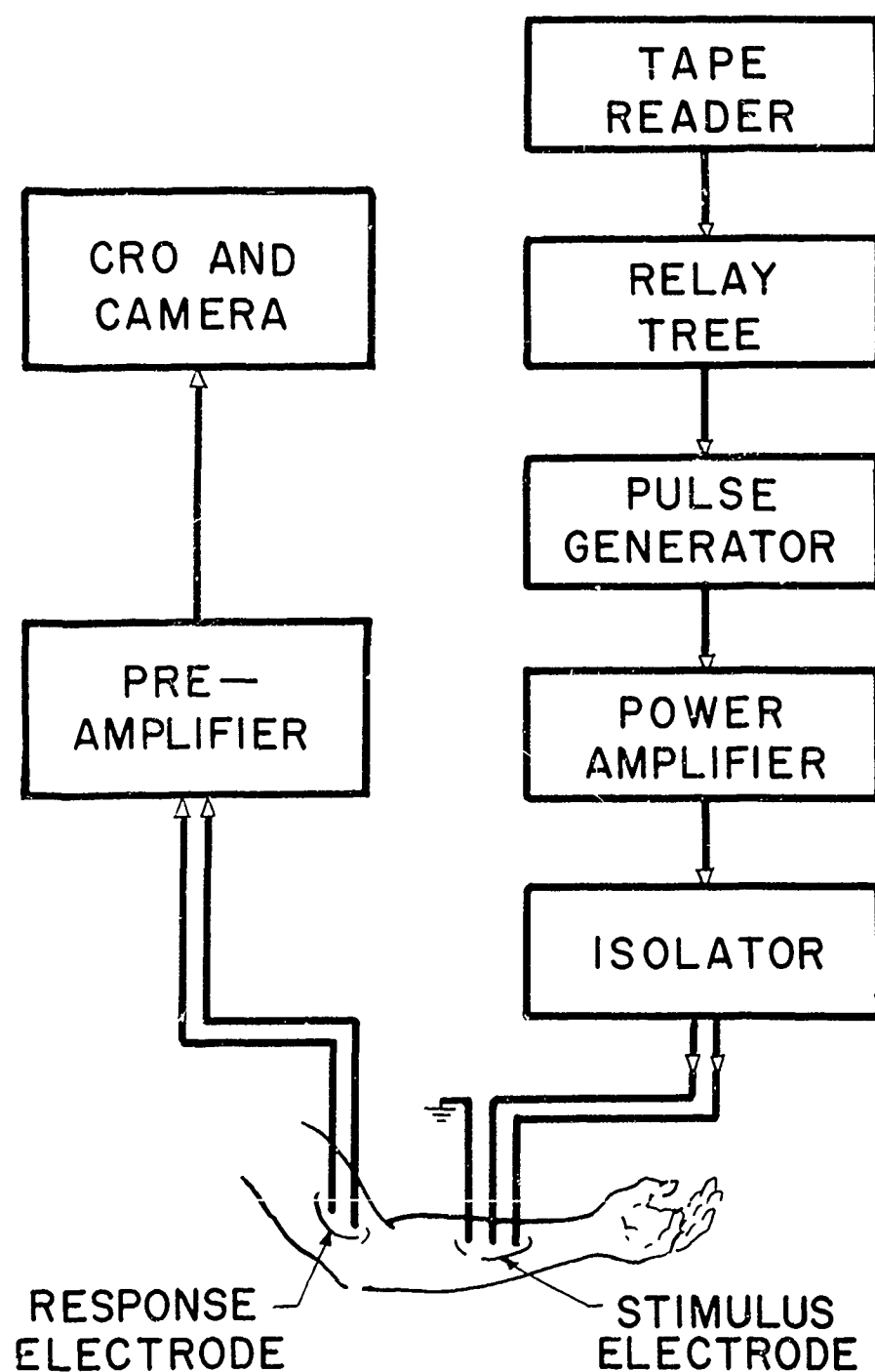


Fig. 1. Block diagram of electronic system showing location of electrodes.

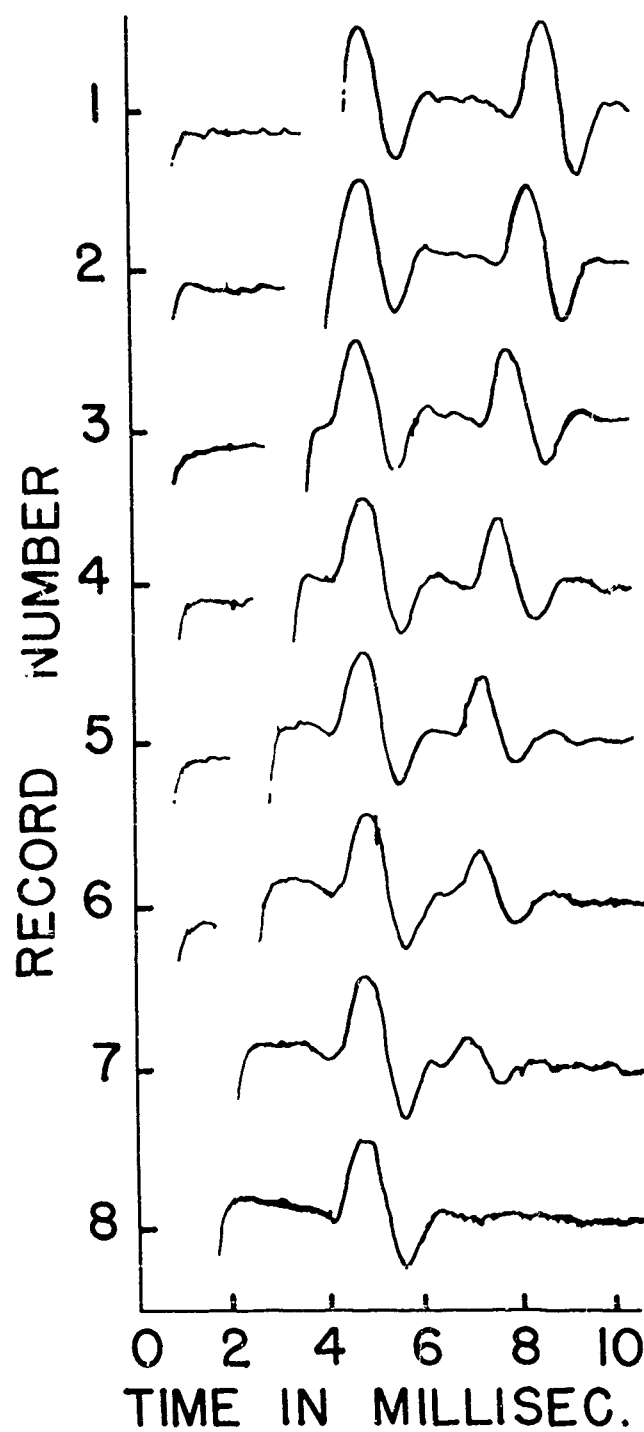


Fig. 2. Sample records showing the decrease in the amplitude of the second response as the interval between the two stimuli is decreased. Every other record from a full series of 15 has been reproduced.

It became obvious in the early stages of the experimentation that many sensory intensity changes occurred as one manipulated the various dimensions of the stimulus. Increasing the current levels and pulse width independently led to the perception of a stronger sensation. Unfortunately both of these two variables had potential artifacts which led us to choose, for our early experiments, the refractory interval as our basic independent dimension. Using this measure, the stimulus pulses are kept constant in amplitude and duration but the intervals between two or more sequential pulses are manipulated. The second of the two responses to these two stimuli pulses occurs in the refractory period following the first response and was found to decrease in amplitude, the shorter the interpulse interval. There was a surprisingly strong following of the psychophysical function associated with this diminution of the second response which we see in figure 2. Here is presented a partial series of the neural responses elicited by pulse stimuli of varying interpulse interval. It is clear that the neural amplitudes decrease as the interpulse interval decreases. This effect is probably due to a decrease (1) in the response amplitude of the individual fiber and (2) in the number of fibers firing. The contribution of each of these two factors remains unknown and future research is required to measure the effect of each. Figure 3 is the plot, for three subjects, of the amplitude of the second neural response.

In figure 4 we have the results of the estimates of magnitude made by the subjects to the same set of stimuli patterns. In this case, the stimuli were presented in a random order to minimize the sequential dependencies. The typical result of this psychophysical experiment is the characteristic S shaped curve shown. The + indicates the estimate of magnitude of a single pulse and it has approximately the same value (though slightly less in each case) as the two pulse situation with very short interpulse interval. Here we have an interesting psychophysiological transform; though the physical energy passing through the nerve is twice that of the single pulse case there is little sensory distinction between the two. The slight amount of difference may be due to the fact that our neurological recording technique is a somewhat less sensitive detector than the subject. Thus slightly suprathreshold responses in a psychological sense do not produce a detectable neural response.

A comparison between the neurological and the psychophysical results can be made with aid of data shown in figures 3 and 4. Fitting a straight line to the segments of the experimentally determined points gives the results shown in figure 5. The horizontal broken line (R₁) represents the amplitude of the first neural response. It remains unchanged and independent of the events following it. The slanted dotted line (R₂) represents the amplitude of the second response which increases with increasing separation. These two curves have been summed and are represented by (R₁) and (R₂) the solid line. The dotted and dashed line is the S shaped curve of the subjective estimates of magnitude. The striking

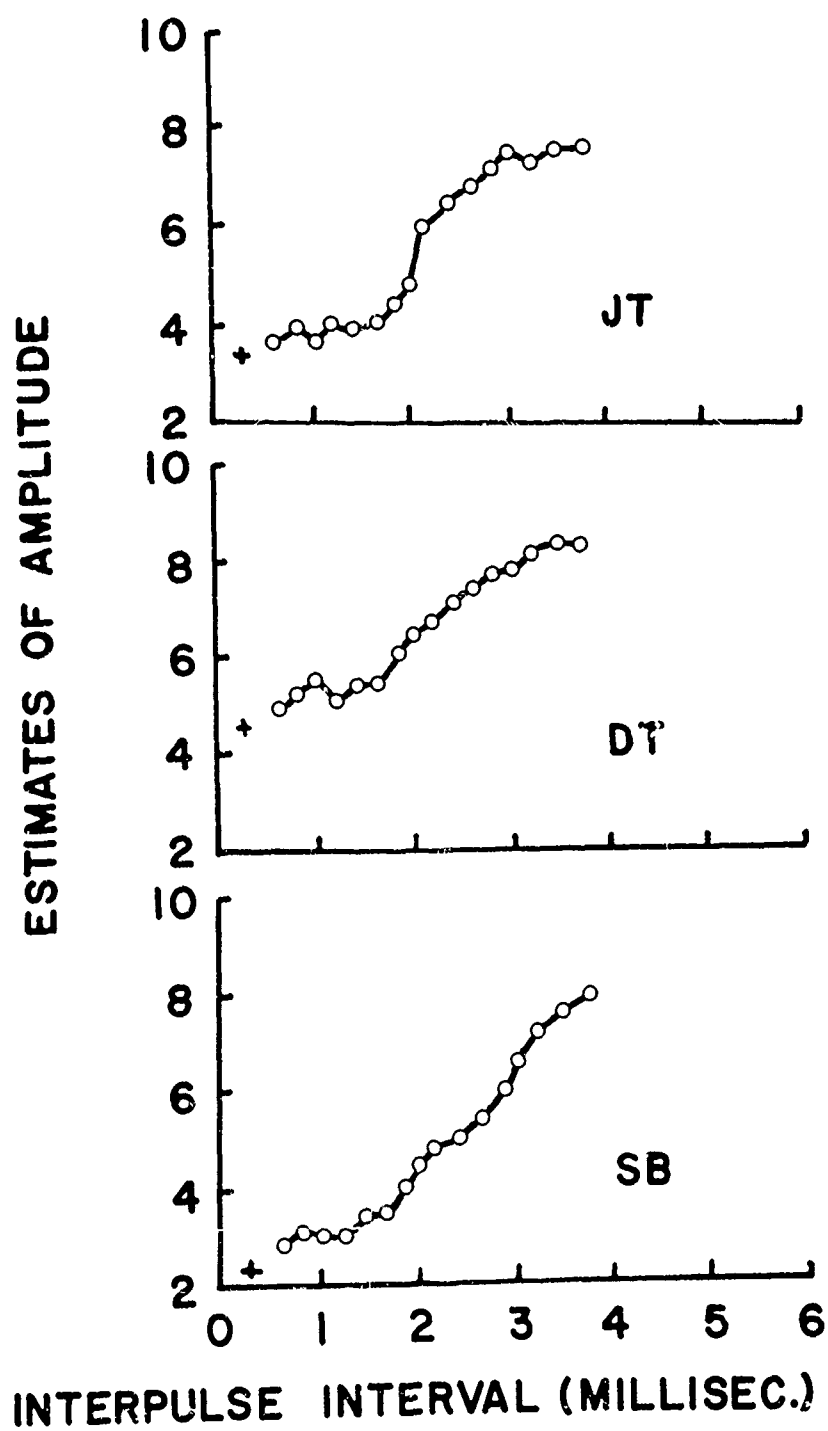


Fig. 3. The average response magnitude of the second neural response as a function of the interpulse interval for three Ss.

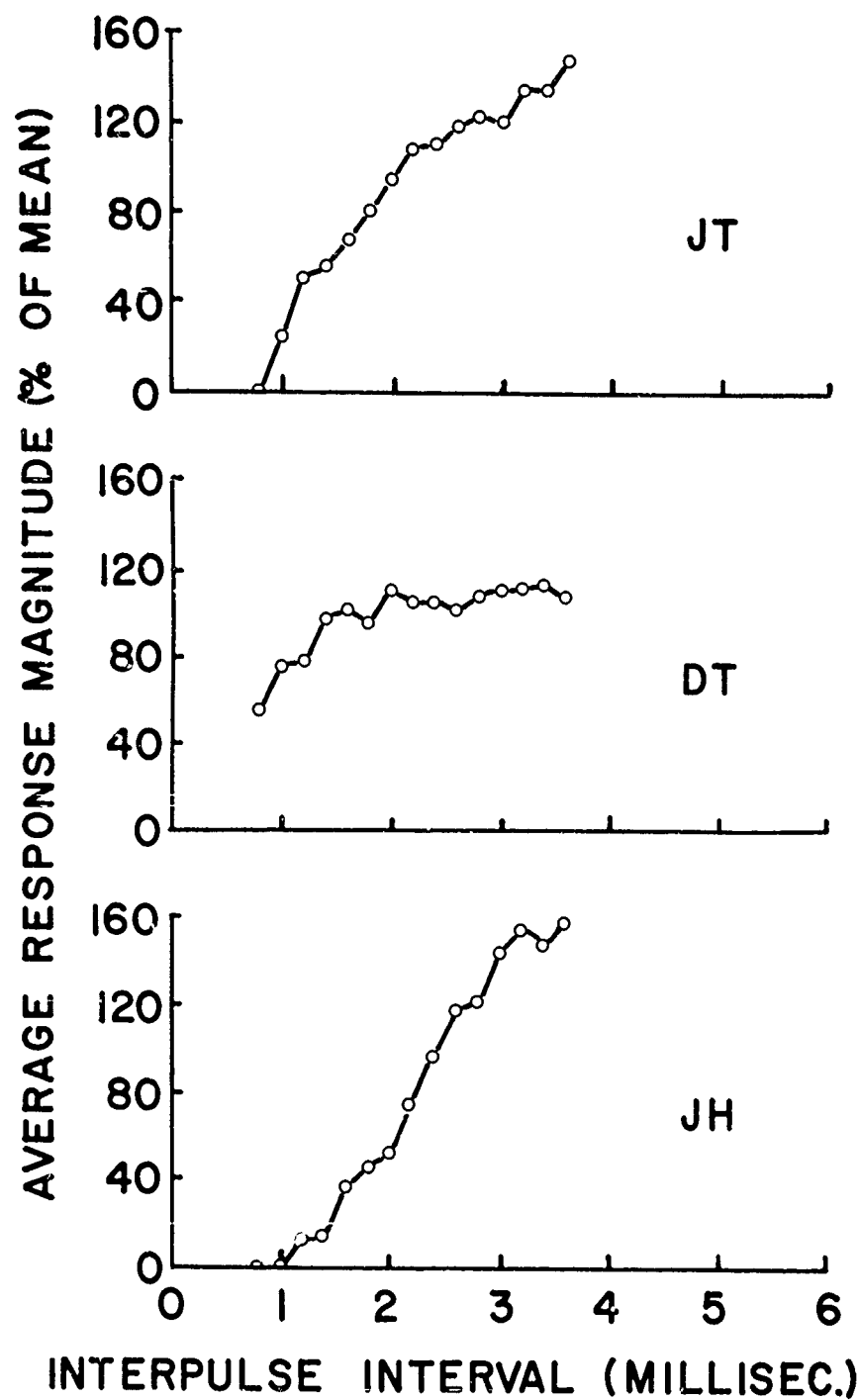


Fig. 4. Subjective estimates of magnitude of the two pulse stimuli as a function of the interpulse interval for three Ss. The crosses indicate the estimates for a single pulse.

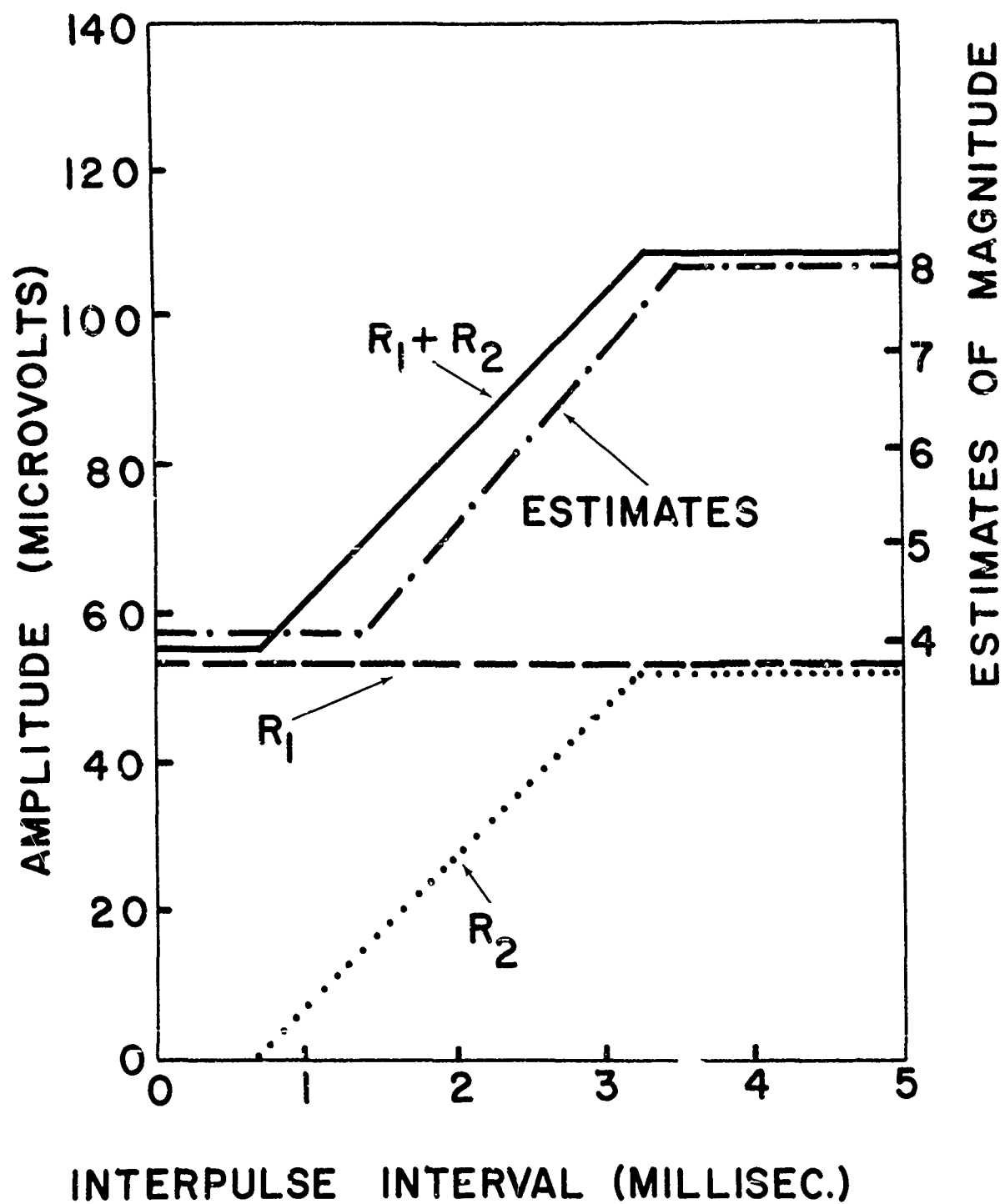


Fig. 5. Hypothetical idealization of the results of this experiment displaying the similarity between the sum of the neural responses and the estimates of intensity.

similarity in shape and points of inflection of these two curves ($R_1 + R_2$) and (estimates) suggested to us that the amplitude of the response was the information carrying measure rather than the clocked time between the sequential pulses. The demonstrations (by Hartline⁴ and others) that increasing the stimulus intensity increased the frequency of response are, indeed, ambiguous on this point for there are two related, but separate, measurements, interspike interval or a count of number of spikes per unit time, which could be made to decode the representation of the physical stimuli intensity. The results of this experiment thus suggest that it is the summation of the amplitudes of the response within some critical intergrating interval that actually is the critical cue. The subjects do report both that the stimulus intensity begins to drop off and that there appears to be a qualitative change-- a detailing of microstructure such as a buzz-- beyond the interpulse interval of 10 milliseconds. These facts suggest that the critical interval in which this integration can occur is of the order of magnitude of 10 ms. If our results can be generalized to continuous sequences of pulses, it may be that the available range of frequencies which meaningfully carry information of intensity would have to be restricted to the range of 333 and 100 cps corresponding to interpulse intervals of 3 ms. and 10 ms. Our estimates of the total information capacity of the nervous system should, therefore, be correspondingly reduced.

Although the correspondences here are seemingly suggestive there is, of course, one uncontrolled variable in this experiment. Both the interval and the amplitudes are changing simultaneously. In fact, we are actually using the interval to change the amplitude. By using three pulses the total stimulus duration can be kept constant and a more clear-cut demonstration may be made of the relationship between summed amplitude and psychophysical magnitudes.

The three pulse experiment was conducted using a modified system which presented triple rather than double pulse stimuli. Here we were able to keep the total stimulus duration constant while manipulating the middle pulse to vary response amplitudes. Table 1 shows the set of stimulus patterns which were used in the experiment. The next plate, figure 6, shows a typical response oscillogram in which one can see the three neural responses and the three stimulus artifacts. It, unfortunately, turned out that the three stimulus artifacts overlapped with the earlier two of the three neural responses, so for some of the stimulus patterns we were not able to collect data on response amplitude. I once again would like to recall to you that the entire triplet occurs within the fusion interval and that this stimulus complex felt to the subject like a single entity without any temporal microstructure.

⁴Hartline, H. V., "Intensity and Duration in the Excitation of Single Receptor Units", J. Cell Comp. Physiol., 1934, 5, 229-247.

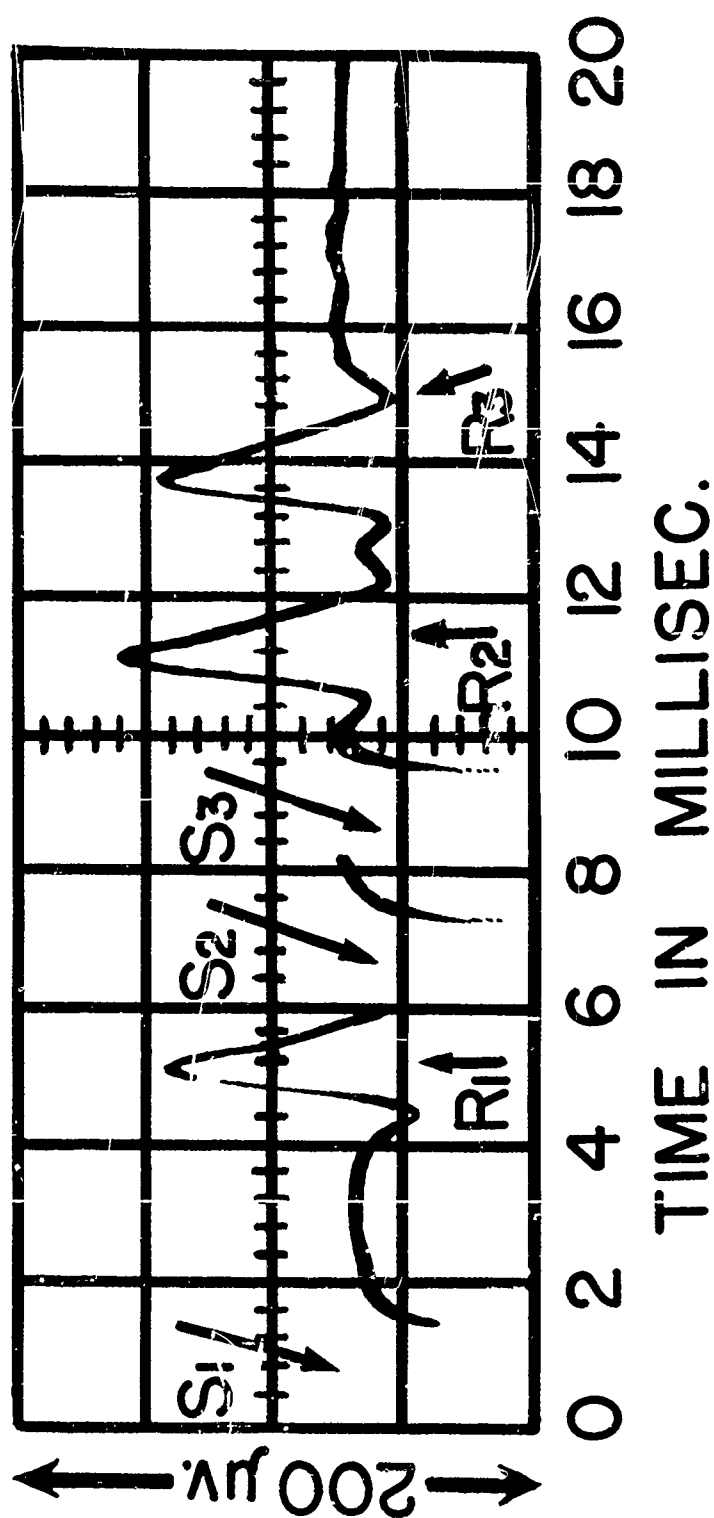


Fig. 6. Typical record with P2 at 6 ms. and P3 at 8 ms. showing the three stimulus artifacts, S1, S2, and S3, as well as the three neural responses, R1, R2, and R3. In this record the trailing edge of R1 has been obscured by S2 illustrating the difficulty in measuring some of the neural response magnitudes.

Table 1

Stim. Config.	Delay of P_2 in ms.	Delay of P_3 in ms.
1	1	8
2	2	8
3	3	8
4	4	8
5	5	8
6	6	8
7	7	8
8	1	7
9	2	7
10	3	7
11	4	7
12	5	7
13	6	7
14	1	6
15	2	6
16	3	6
17	4	6
18	5	6
19	1	5
20	2	5
21	3	5
22	4	5
23	1	4
24	2	4
25	3	4
26	1	3
27	2	3

The 27 different stimulus configurations used in this experiment.

Once again we summed the amplitude of the three neural responses to give an integrated measure of the total response activity.⁵ The suitability of this measure may be shown by a comparison of figures 7, 8, and 9. We abstracted from the two pulse experiment an analytical expression for the amplitude of the second response as a function of interpulse interval.

⁵Joel Brown, working in our lab, was able to show a linear relation between the peak to peak amplitude measure and the area as measured with a planimeter. Some subjects do show a desynchronizing for high stimulus intensities but it appears to be a minor perturbation.

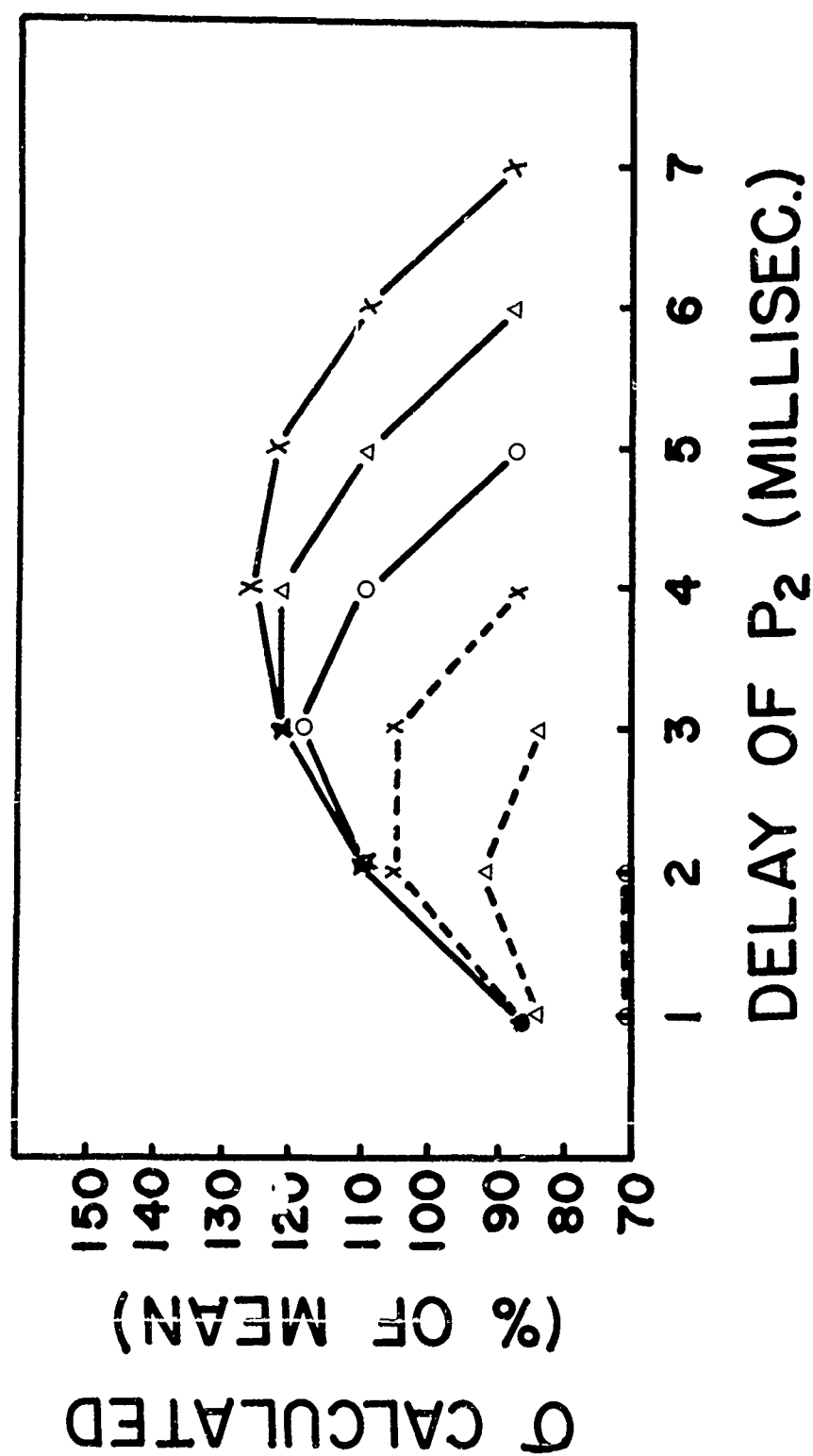


Fig. 7. Subjective estimates of magnitude of the three pulse stimuli as a function of the 27 stimulus configurations. The family of curves has been partitioned on the basis of the position of P₃.

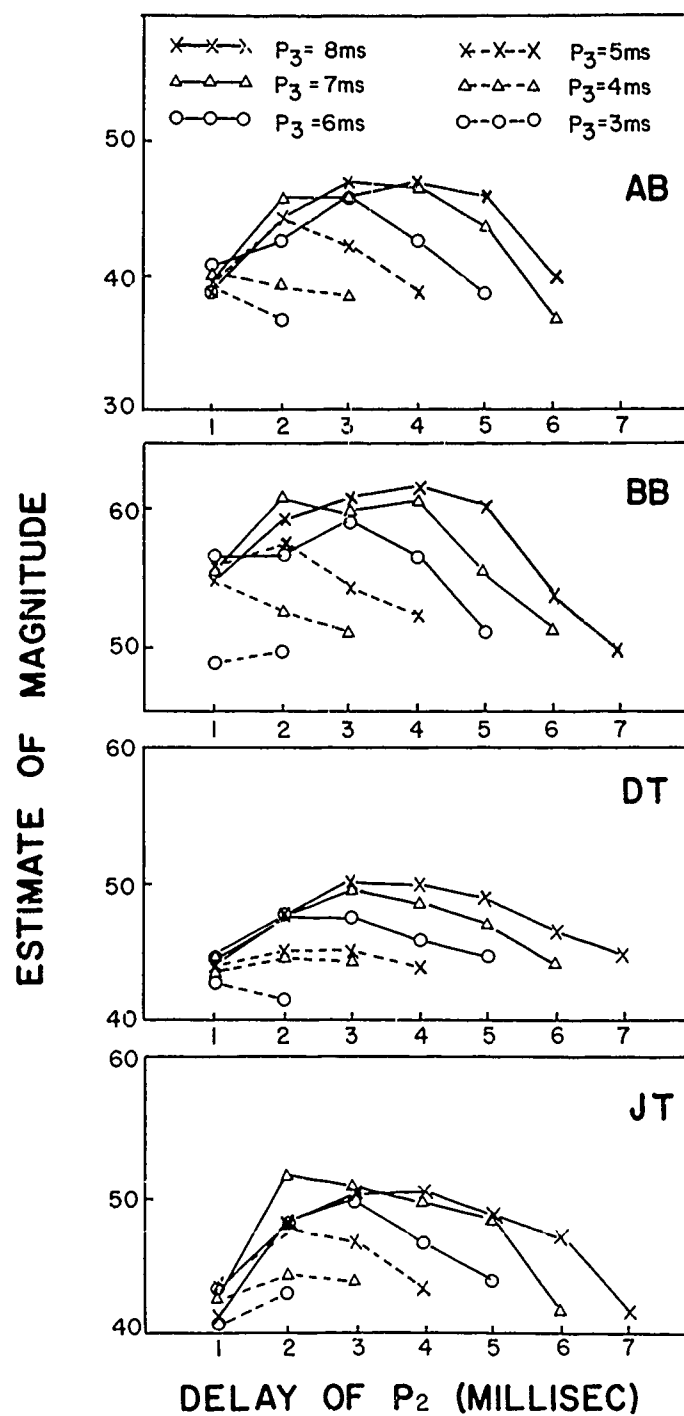


Fig. 8. The sum of the three neural response amplitudes as a function of the 27 stimulus configurations. The sums have been plotted as percentages of the mean of the sums.

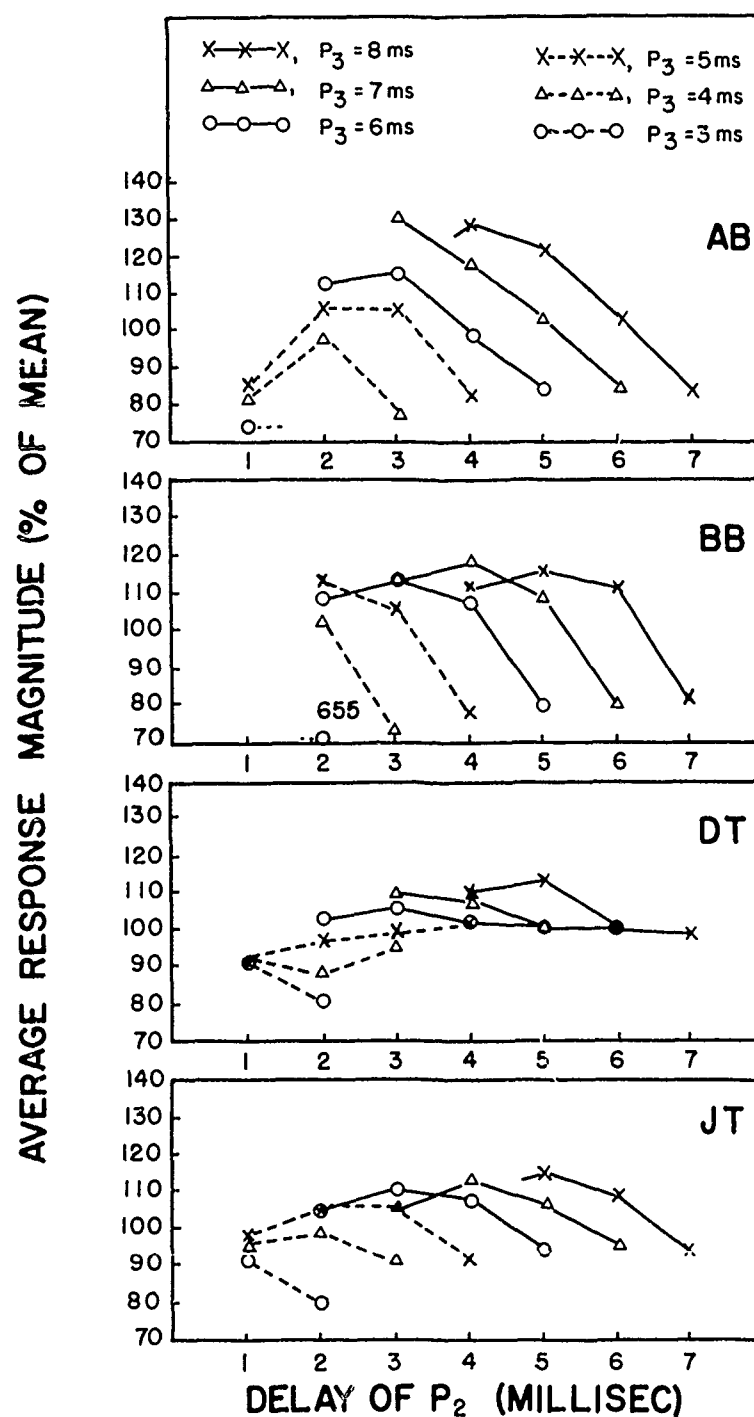


Fig. 9. Calculated sum neural amplitudes. The sums have once again been plotted as percentages of the mean of the sums.

This is expressed by the following relationship:

$$A = \begin{cases} .416 t - .333, & 0 \leq t \leq 3.2 \text{ ms} \\ 1 & , \quad t > 3.2 \text{ ms} \end{cases}$$

If we were to consider the relationship between the amplitude of the second response and the time between it and the first pulse, the amplitude of the third response and the time between it and the second response and finally the constant amplitude of the first pulse, we can develop the following expression:

$$a = N_1 + N_2 f_1(t_1) + N_3 f_2(t_2)$$

where N_1 , N_2 and N_3 are the 100% levels of the three responses, is the integrated response amplitude and:

$$f_1 = \begin{cases} .416 t_1 - .333, & 0 \leq t_1 \leq 3.2 \text{ ms} \\ 1 & , \quad t_1 > 3.2 \text{ ms} \end{cases}$$

$$f_2 = \begin{cases} .416 t_2 - .333, & 0 < t_2 \leq 3.2 \text{ ms} \\ 1 & , \quad t_2 > 3.2 \text{ ms} \end{cases}$$

Let us now substitute in this equation all of the combinations of values of (the time between P_1 and P_2 , and P_2 and P_3 , respectively) t_1 and t_2 which we have used in our experiment and generate a family of curves which vary with the position of P_3 . Figure 7 shows this theoretical plot of the amplitudes. Now let us go on to the integrated neural amplitudes as directly measured from our oscilloscope pictures. Figure 8 shows that plot. As we mentioned before, some of the data are missing from the left hand side when the stimuli artifacts obscured the responses, but generally we see that the form has been well predicted. There is a cluster of points at the values of P_2 following P_1 by one ms. and the line is relatively flat across the points representing P_3 following P_2 by 1 ms. There is a considerable amount of variation between subjects, but it is my feeling that this is primarily a function of the artifacts introduced by electrical stimulation. Let us now turn to figure 9 which shows the estimates of magnitude the subjects made to the same stimuli patterns. The correlation between this figure and our predicted neural amplitude is actually far better than between our predicted and actual neural amplitudes. Notice the points which have been subjectively equated in amplitude (indicated by, once again, the cluster of points where P_2 followed by P_1 by 1 ms. and the flatness across the points representing P_3 following P_2 by 1 ms). These points actually represent situations in which the stimuli configurations have varied over the total range used in the experiment. In other words, even though we varied the spacing considerably, there was no effect on the psychophysical

amplitude until there was a degradation in the response amplitude due to the refractory period. Since this time region lies right in the middle of the frequency domain in which the spacing would have to vary to serve as a critical measure of sensory intensity, it is clear that the temporal spacing is not conveying information, i.e., not being measured or acting as a code. The neural amplitude, however, does predict to a high degree the psychophysical magnitude. The correlations between the neural amplitudes and the psychophysical estimates range between .82 and .89 for these four subjects. Thus we may conclude that the neural code for intensity is in some way related to the amount of neural activity and that it is more probably a count of the number of responding fibers and the number of responses in a unit time rather than spacing information which serves as the critical cue in intensity coding.

Since this meeting is primarily directed toward the use of somesthesia as a communication system, I would also like to discuss a series of experiments on spatial interactions which not only supplement the results of the temporal interaction studies described above but also are pertinent to spatial codes. In some of our earlier studies, we set up an array of stimulating electrodes on the surface of the forearm. It was immediately apparent that two interesting phenomena were expressing themselves. The first showed that the Weber Circle, for electrical stimulation, is, in fact, a long thin ellipse. If the two point threshold were measured between two electrodes separated laterally on the arm, a much smaller distance was required for accurate localization of electrical stimuli than when the test was made with longitudinally separated electrodes. This, of course, must be related to the longitudinal nature of the nervous supply to the arm.

In pursuing this study we also notice that there was a generalized tendency when two simultaneous stimuli were presented for one to block or mask the sensation of the other. On the forearm this problem was complicated by the merging of stimuli into single sensations so our results seemed inconclusive. (Need I point out that Professor v. Békésy has ingeniously analyzed these mixed signals?) To us, however, this merging prevented the measurements we were most interested in and for this reason we went to the fingers to further pursue this inhibitory interaction and to see if possibly we could measure the gradient of interaction between stimuli. Figure 10 shows the stimulating apparatus which was used to carry out this investigation. The stimulators in this experiment were new units which provided a constant current rather than constant voltage pulse used in the earlier experiments and thus allowed us to manipulate a stronger masking pulse and a threshold test pulse. These were applied to the finger tips through dime-sized stainless steel electrodes. These pulses were separated in time by one half of a ms. to avoid electronic beating in the isolation stages of our stimulators. The masking stimulus was kept constant from day to day, at one of three different levels--4, 3, or 2 ma. A modified method of limits was used to determine the absolute threshold under the various conditions of masking and

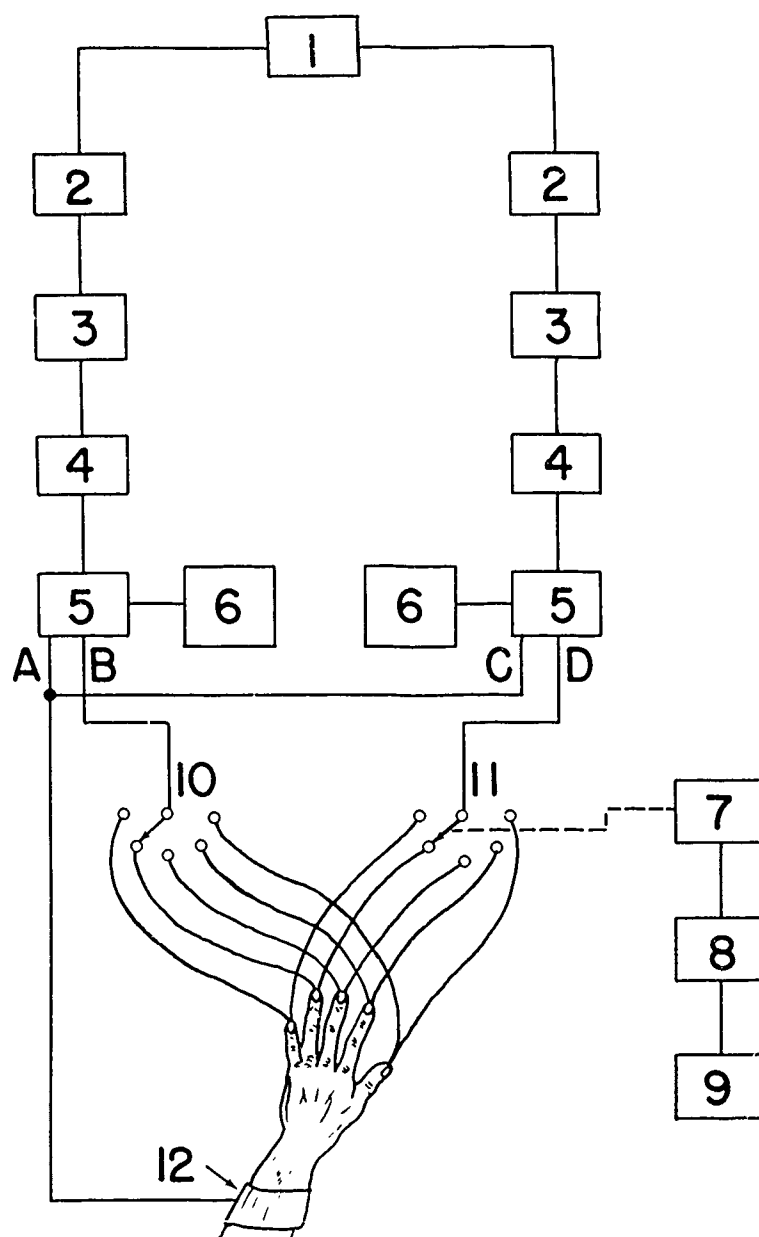


Fig. 10. Block diagram of the electronic circuit showing (1) the master pulse generator, (2) the secondary pulse generators (3) the power amplifiers, (4) the isolation units, (5) the constant current units and (6) their power packs, (7) the relay tree, (8) the punched paper tape reader, (9) the summary card punch, (10) and (11) the selector switches and finally (12) the common positive electrode strapped to the arm.

also with no masking. In figure 11, we see the results of this experiment. The shaded bar in each case represents the finger in which the masking stimulus occurred and the points indicate the thresholds as measured in the other fingers. The lower right hand graph is a measure of the absolute threshold under conditions of no masking and shows a curious effect. The threshold to these constant current pulses is considerably less for the little and ring finger than for the index finger and thumb. Since our stimuli were constant current in form, it is unlikely that this is an artifact, but represents a true neurological phenomenon, one whose adaptive significance is not at all clear since the index finger and thumb are associated with fine manipulation.

We have turned to neurological experiments done on cats and monkeys for clarification of the spatial interaction data from our masking experiment. Towe and Amassian⁶ performed an analogous experiment on monkeys; they recorded the response of a somatosensory cortical cell, which previously had responded to stimulation of one digit on the monkey's hand, and at the same time presented a masking stimulus in an adjacent finger. The result was a blocking of cellular response for long periods of time. This blocking effect decreased as the masking stimulus moved to a more distant finger. This result is concordant with our psychophysical data, and I feel supports the notion that sensory intensities are coded by a counting procedure rather than by an interval measurement. A three-way correlation exists between increased threshold, stimulus condition and the probability of a cortical cell firing or not.

Further evidence for this counting concept lies in an elegant experiment by Li, Cullen, and Jasper.⁷ In this experiment they applied two pulses of variable spacing to the thalamic relay for the somatosensory cortex. Although, unfortunately, their intervals range from 5 to 220 ms, and thus do not cover the region less than 10 ms, as tightly as we would like, there are, for us, a number of interesting parts of their observations. Figure 12 is a reproduction of their results. You can see the increase in the number of spikes during the period we called the integrating interval. No time separation between the sequential responses can be detected. This, interestingly enough, is the region in which we had complete fusion. In records three and four there begins a separation between the sequential sets of responses to the two stimuli. This is also approximately the region in which the subjects in our experiment begin to feel temporal microstructure reported by them as a buzz

⁶Towe, A. L. and Amassian, V. E. "Patterns of Activity in Single Cortical Units Following Stimulation of the Digits in Monkeys", J. Neurophysiol., 1958, 21: 292-311.

⁷Li, Choh-Luh, Cullen, C., and Jasper, H. H. "Laminar Micro-electrode Studies of Specific Somatosensory Cortical Potentials," J. Neurophysiol., 1956, 19: 111-130.

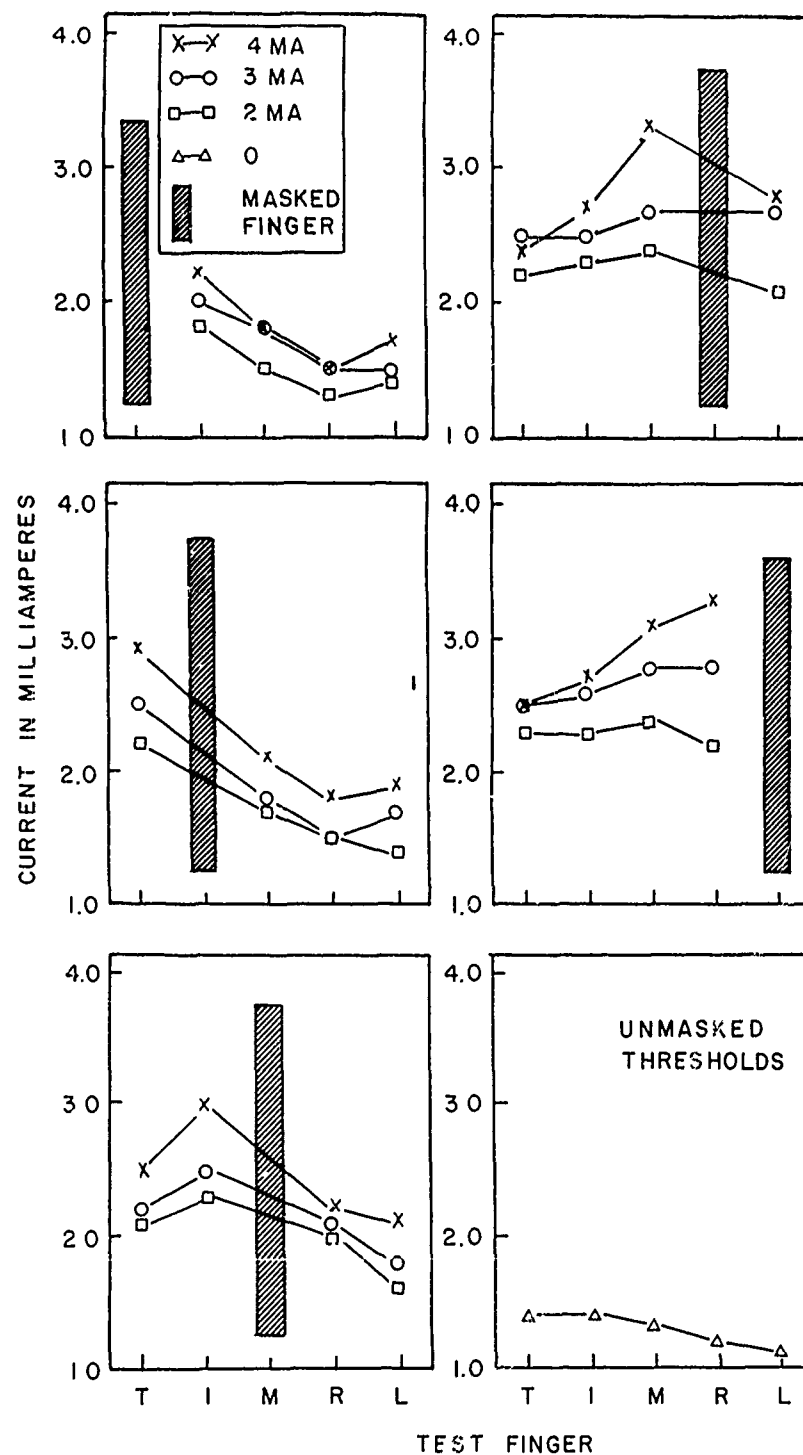


Fig. 11. The results of the experiment for S JT. The horizontal co-ordinate is in terms of the stimulated fingers. T = Thumb, I = Index Finger, M = Middle Finger, R = Ring Finger and L = Little Finger.

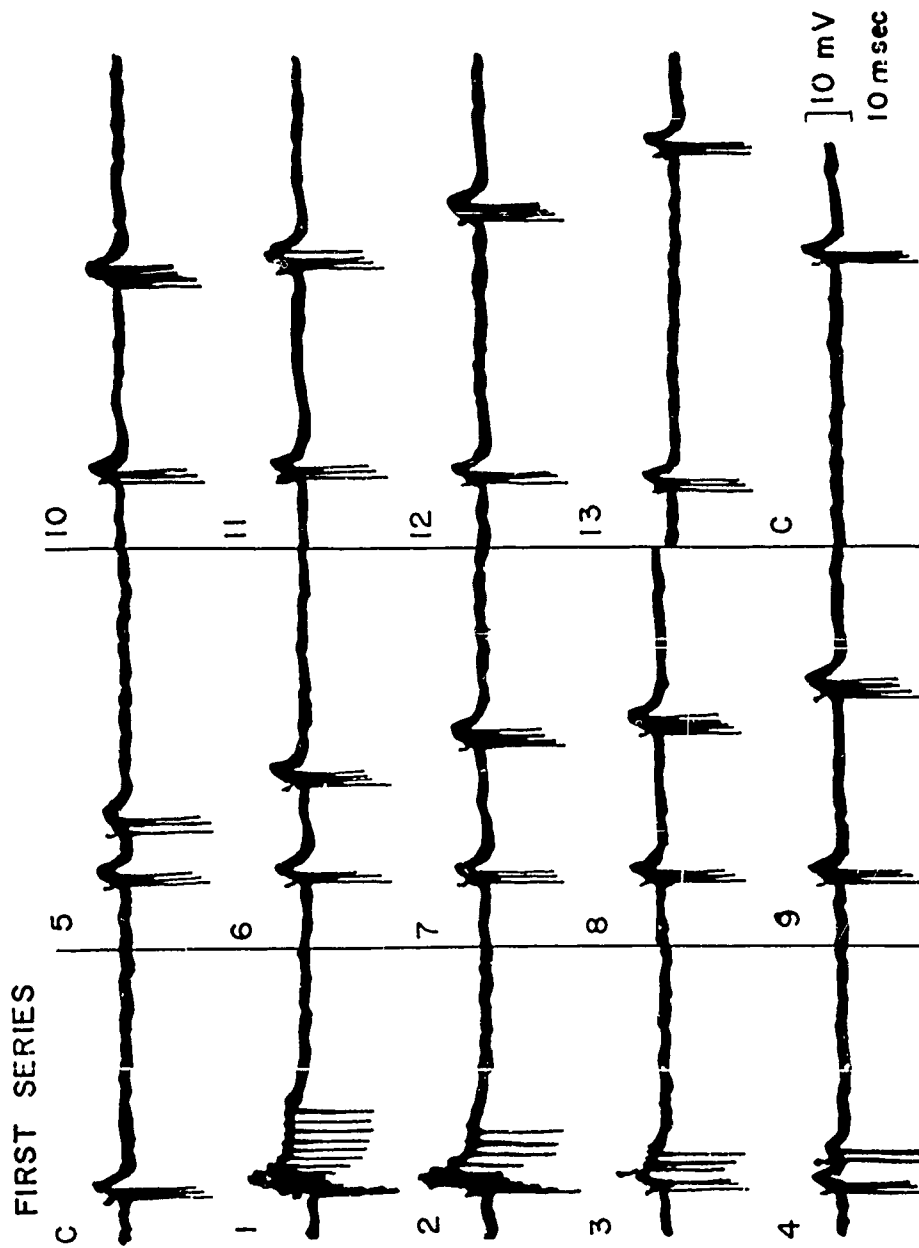


Fig. 12. Single cortical cell response to dual shocks applied to thalamic relay for somatosensory cortex. Intervals are (1) 5 ms., (2) 8 ms., (3) 20 ms., (4) 47 ms., (5) 45 ms., (6) 84 ms., (7) 117 ms., (8) 128 ms., (9) 160 ms., (10) 167ms., (11) 180 ms., (12) 220 ms., (13) 290 ms., (C) Control. From Li, Cullen and Jasper⁷.

or a resolution of the two pulses. As the two stimuli move further and further apart, the number of spike responses to the first stimulus drops off, then increases slightly, and finally levels out at the original level. In some of our pilot studies on two pulse stimulation with long separations we have found the subjective magnitude of the second stimulus drops off after 10 ms. and remains relatively constant for intervals up until a second. This experiment is a difficult one to interpret, however, for there is a quality change as well as a quantity change above 10 ms. If one limits the observations to separations greater than 50 or 75 ms., guaranteeing that the pulses are all well resolved, all subjects will feel two separate responses. If asked to scale the second of the two, he will report the perceived magnitude to remain constant throughout the range. In Li, Cullen, and Jasper's experiment we see, once again, extreme temporal changes with no correlated changes into sensory magnitude observable in our psychophysical experiments.

We are now, at IBM, investigating the evoked cortical response which is produced by these same pulse electrical stimuli. We have adapted some of the techniques developed by W. Rosenblith⁸ at M.I.T. for acoustical stimuli to somatosensory phenomena. Electrograms are recorded on analogue magnetic tape along with marking signals which control the action of an automatic digitizer. These digitized records were placed on digital magnetic tape and averaged to pull out of the physiological noise the slight evoked potentials. We feel that this shall provide an important avenue of research on intact human beings allowing us to probe at another level of the nervous system while subjects remain able to make psychophysical judgment. Figure 13 is a typical averaged evoked potential from one of our subjects, showing the first 100 ms. of the record. The response, which lasts up to 300 ms. seems to be composed of three parts: A very long lasting wave we have referred to as the O wave and two shorter spikes, the first we called the M wave and the second we called the N wave. We have not yet established the significance of these various components but we have established that the entire system saturates at low stimulus intensities and that the psychophysical and electrophysiological thresholds are very close together. I hope that shortly we will be able to report more on the details of this potential with particular emphasis on its significance as an indicator of the psychological state.

I have spent a great deal of time discussing the coding phenomena of intensity. I would like now to turn briefly to the problem of quality coding in somethesis. We enter here a field in which we, from the outset, are burdened with a lack of quantification of one of the important variables. The central question in quality coding, of course, is what spatial or temporal neural patterns are correlated with our ability to discriminate between thermal, mechanical and noxious stimulation.

⁸Processing Neuroelectric Data, Tech. Report 351, Research Laboratory of Electronics, M. I. T., July 7, 1959.

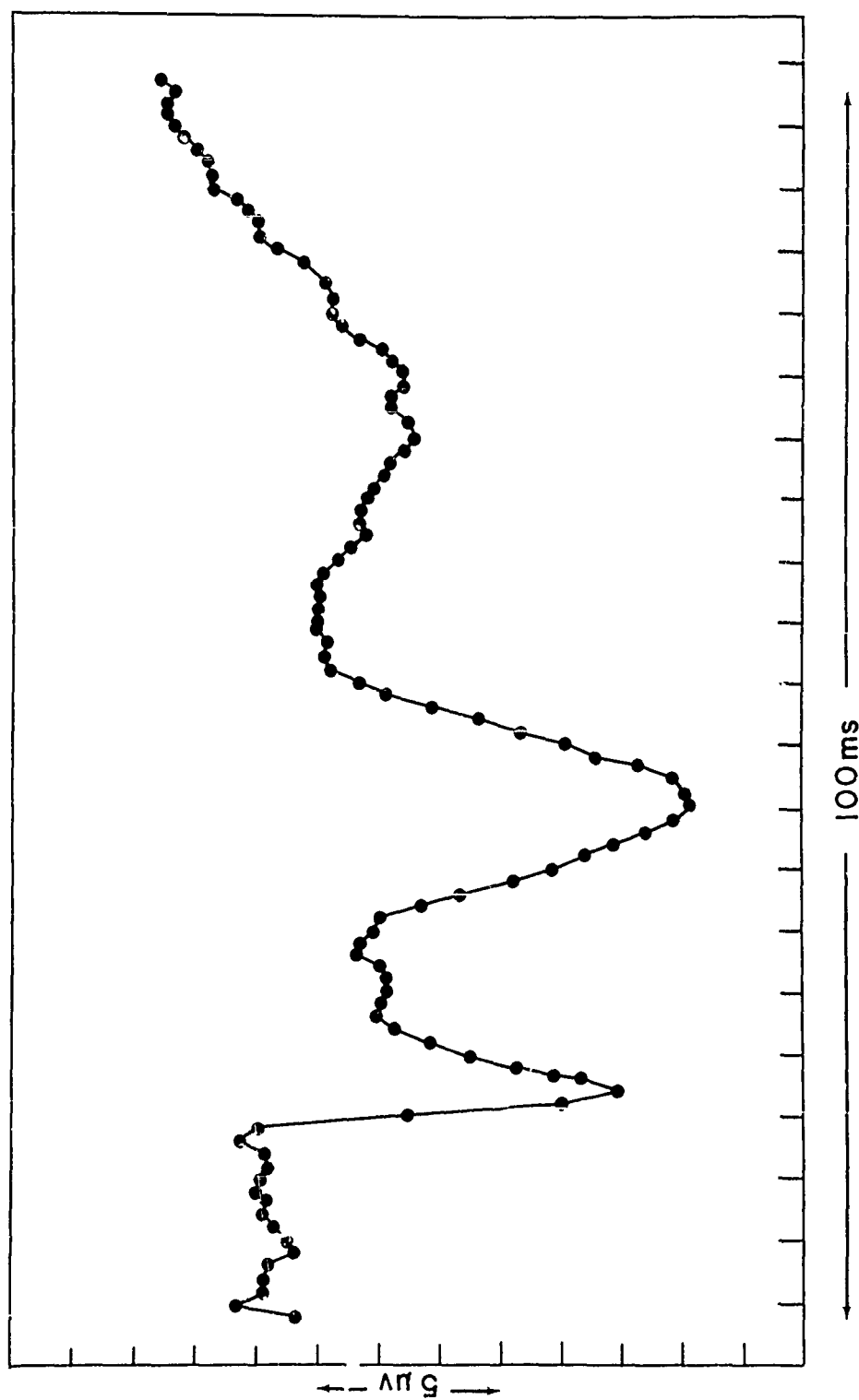


Fig. 13. Averaged evoked cortical response from the somatosensory cortex of man produced by electrical shock to median nerve. Eighty separate records have been averaged to give this record.

Even though we may agree that there is no metric to sensory dimensions and debate the existence of measures of these dimensions, there is a more fundamental problem, i.e., are we all talking about the same dimensions themselves. It seems that the number of cutaneous senses is not one of unanimous agreement.

There are some recent developments which might clarify this issue. Perhaps one of the most fundamental questions is the one of specialized or generalized receptors. The early microscopic investigations of the skin showed a variety of specialized structures which have been, from time to time, correlated with one or another sensory qualities. The Pacinian and Meissner corpuscles were associated with touch, the Krause and Ruffini bodies with thermal sensation, and, of course, the free nerve endings are said to mediate "pain". Recently, in England, a new theoretical view has been expressed by Weddell and his colleagues which suggest that there are no specialized receptors but that the nerve terminations in the skin merely respond differently to different stimuli conditions. Thus, all of these modalities could be carried upon a single nerve fiber coded by different patterns of firing. Others with equal vigor still support specialized receptors as the coding modality.

I would like to call your attention to an experiment of great elegance which should modify some of our views concerning the Pacinian corpuscle, at least. This onion-like encapsulated nerve ending has long been associated with touch sensation and has been shown to be extremely sensitive to mechanical stimulation. Lowenstein⁹ having isolated one of these structures, proceeded to stimulate the corpuscle in sequential stages of undress by peeling away the layers of the corpuscle. In some striking oscillograms, it is clearly shown that there is absolutely no change in the response or the sensitivity of the underlying neuronal filament even when the corpuscle was entirely dissected away. Thus, the corpuscular structure so long identified as the end organ for tactile sensation is shown to have little to do with the actual transduction of mechanical to electrical energy. Lowenstein indeed did find sensitized spots on the end portion of the nerve which did seem to generate a graded potential which was correlated with the ensuing spike potentials. What then is the function of these corpuscles? Wall¹⁰ has pointed out that these nerve endings very often lie at the periphery of the body exposed to radiation and mechanical damage. These extrinsic factors coupled with the very fact that these terminations themselves represent in some sense an abnormal boundary condition of the nerve suggest that these encapsulations may be tiny neuromas--growths or tumors of non-adaptive significance, rather than functional receptor units.

⁹Lowenstein, Werner R., "The Generation of Electric Activity in a Nerve Ending", Ann. N. Y. Acad. Sci., 81, 2, 367-387.

¹⁰Wall, P. D., Personal Communication.

Wall¹¹ has further proposed an interesting theory of quality coding. He has shown that second order spinal neurons have impinging upon them, fibers carrying information of all of the cutaneous modalities. The distinctive difference is that the fibers react differently temporally to each type of stimulation. In some equivalent, though recorded, form then this pattern of activity is carried intact towards the cortex and decoded there into the parameters of sensation. Pfaffman¹² recently has described just the same sort of thing for taste perception in which relative rates of firing are the key cue.

What then may we conclude from these findings? First of all, the results of our experiments have shown that temporal factors seem to be unimportant in the coding of sensory intensities but that a count of the number of impulses, either spatially distinct and temporally synchronized, or sequential events on single lines, is correlated with fluctuations in sensory intensity. On the other hand, physiologists seem to be challenging the concept of separate lines of spatial coding as the foundation of quality coding. They appear to be turning to temporal order or pattern to explain quality coding. These new results, of course, represent a complete reversal of the classic concepts which hypothesized parallel lines for separate modalities and time rate of firing as the intensity cue.

We are tantalized by some of these results and stimulated by these theories. The 180° reversal of time and space warns us to be cautious. Yet, I feel that boldness, particularly in pursuing psychophysical-neuro-physiological comparisons on man will enormously accelerate the final resolution of some of these unanswered but fundamental questions.

¹¹Wall, P. D., "Two Skin Sensory Transmission Systems", Symposium on Sensory Communication, Wiley, to be published in 1960.

¹²Pfaffman, Carl, "The Afferent Code for Sensory Quality", Amer. Psychol., 1959, 14, 226-232.

The Influence of Inhibition on the Sensation
Pattern of the Skin and the Eye

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In the seventeenth century the concept of independent and dependent variables was introduced into the natural sciences. This made it possible to describe physical phenomena in the form of functions. The advantages were so great that the same concepts have been applied since to all parts of science, applied science, and even psychology. In the field of communication engineering this kind of thinking led to a transmission scheme such as is shown in Figure 1. On the left side a generator is connected to input of the system under investigation. On the

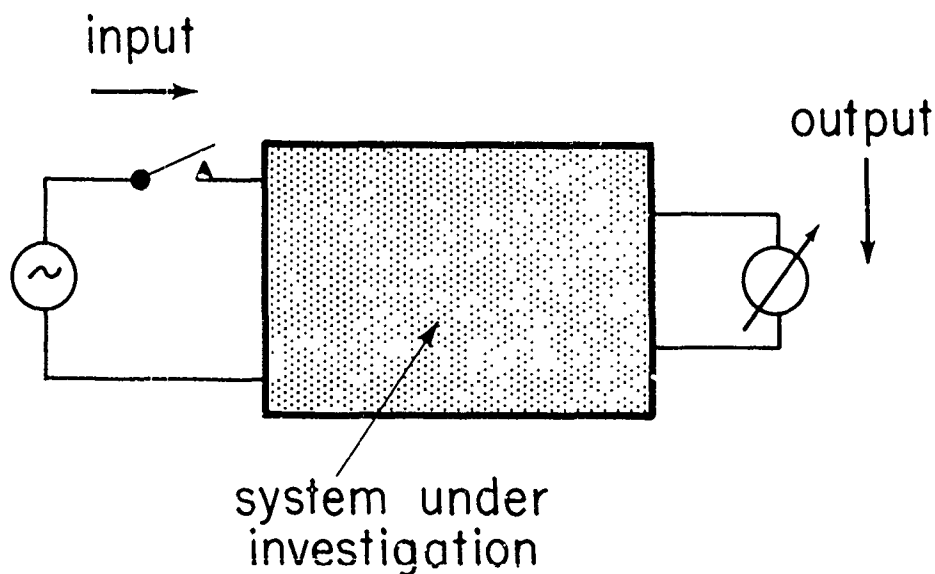


Fig. 1.

right side the output of the investigation system is connected to an energy consumer. Thus we have a black box with four terminals -- two input and two output. The modern development of the filter theories and transmission systems is closely connected with the introduction of this simplified scheme. This scheme was so successful that it has been applied to living systems. And thus the concept of function has been introduced into biology, and the living system has been schematized

as a black box with two input terminals for stimulus and two output terminals for the production of sensation or any other biological effect.

The question arises, to what degree is this simplification possible? It is obvious that the living tissue is represented by a passive system. Let us consider first the question of energy transmission. Since no energy is produced in the passive system, the input energy has to be the same as or larger than the output energy. At the same time the black box has to have dimensions that can transmit this amount of energy. In general, for large amounts of energy this would make the biological system quite clumsy, and you can understand that a passive system exists in nature only in situations where the energy input and output are continuous. But since we use our muscles, and other faculties only for short lengths of time, nature has developed most biological systems for interrupted use. Thus it is possible to accumulate large amounts of energy, trigger them at the needed time, and release them to the output. By doing so, the triggering process produces the stimulus. The energy is supplied to a black box continuously in another way but not through the input generator pictured at the left of Figure 1. It is well known that the nerve system works that way by producing short electrical spikes. The muscle contractions and most of the secretions are interrupted outputs.

A second consideration that makes it difficult for nature to use a simplified black box is the fact that any mistake in the output could damage the biological system. For instance, if we make a wrong judgment in the movement of the hand, it may hurt the hand. Therefore in the biological systems we need a sort of feedback that controls whether the output performance is carried out properly. This is one more complication that has to be introduced to avoid damaging the system.

The introduction of a feedback system in the biological system necessarily involves some sort of switching device that permits adjustment of and timing of the different outputs. In biology this device might be called inhibition. For instance, if we move an arm from one place to another, it is necessary to contract one muscle and at the same time to inhibit the antagonistic muscle. In the biological system every output is produced by a stimulation connected with simultaneous inhibition.

It is usually assumed that the inhibitory effects are small side-effects, but the fact that any movement of one muscle is connected with inhibition of another muscle shows that the inhibitory effects are not a small phenomenon. In the paper that follows I should like to show some of the inhibitory phenomena that occur in the stimulation of sense organs with large receptor surfaces. Of particular importance in this respect

are such sense organs as the skin, the eye, and the organ of Corti in the ear. Some of the inhibitory phenomena are capable of modifying the sensation pattern appreciably, compared to the sensation pattern that would be predicted by the assumption of a simple passive reaction of biological sense organs.

It was probably Mach (1) who first demonstrated such a modification of the sensation pattern. For some of his stroboscopic experiments he cut from black paper a disk with the pattern shown in Figure 2. If this disk is rotated at a speed at which flicker disappears the light intensity is high in the center and diminishes toward the edges of the disk. The

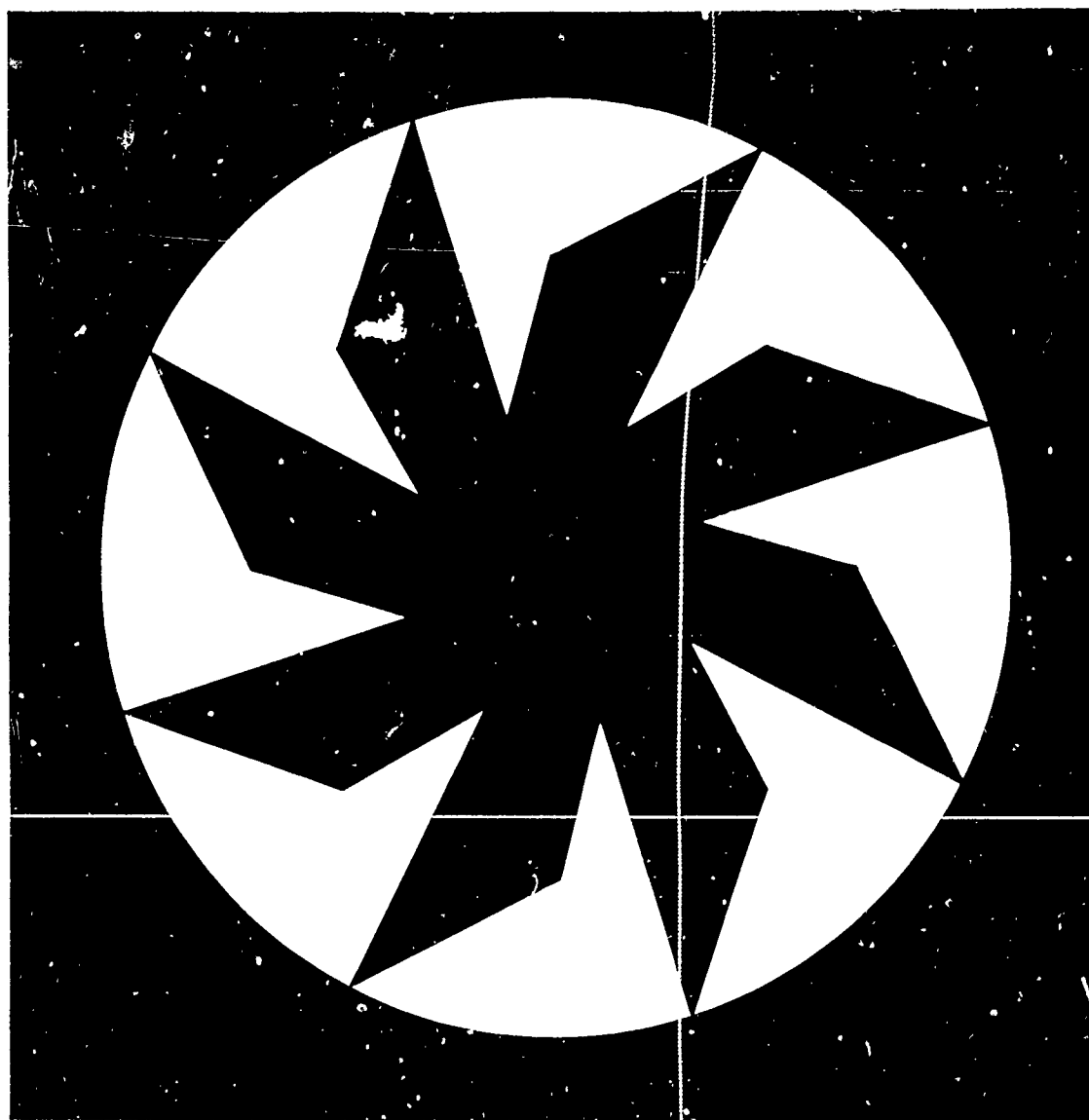


Fig. 2.

distribution of the light intensity from the center toward the edge is shown by the solid curve in Figure 3. The dotted curve represents the distribution of sensation intensity as seen by the eye. It is surprising

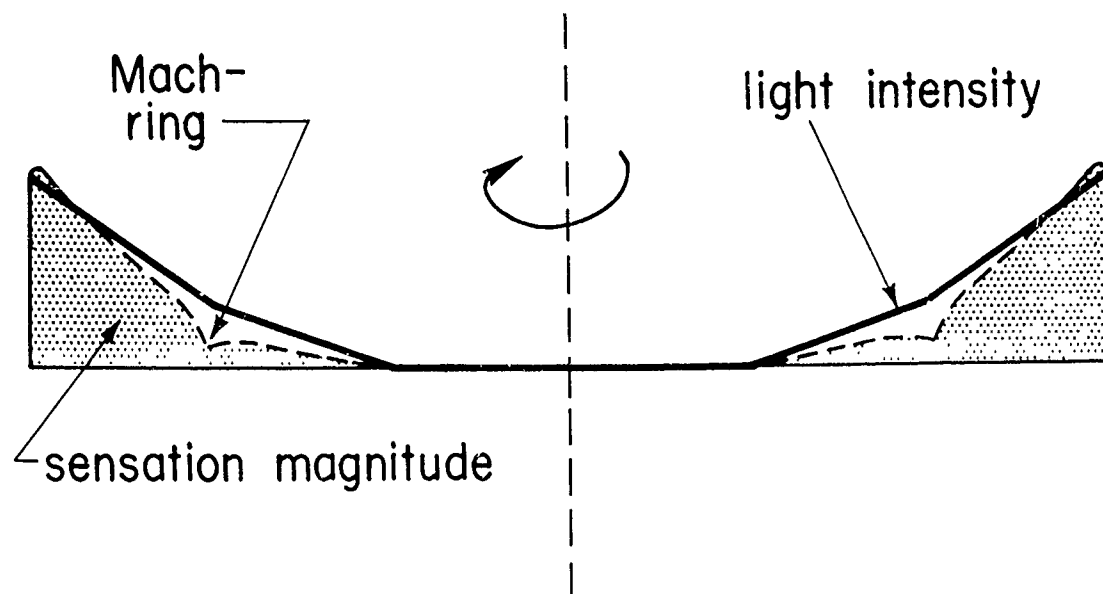


Fig. 3.

to realize that, every time there is a break in the continuity of the distribution of light intensity, there is an additional increase or decrease in the sensation of light magnitude. This phenomenon is shown in Figure 4, which represents the disk of Figure 2 during rotation.

Another way to show that the inhibitory effects on the retina are not small can be obtained by observing a small illuminated point. It is well known that we see the stars sharply defined as small points, but it is equally well known that a light beam entering the eye is, to quite a large extent, diffused. This suggests that around the sharp image of the star we should see a sort of halo. On preparations of the eye, this halo around the sharp image of the points is easily observed, but if we look at the stars we do not see the halo; therefore the halo around a strongly illuminated point must be suppressed by inhibition (2). We can show by an experiment the magnitude of this type of inhibition on the retina. For this purpose we move one bright spot along the surface of the retina. As is seen in Figure 5, this bright spot has a sharp maximum value at most points of the retina, but if the same spot is moved into the region of the blind spot of the eye, the lateral inhibitory phenomenon disappears and we suddenly see the sharp spot disappear and in its place a large halo appears in the eye. To carry out this experiment we need only fix one eye on a small sharp point of

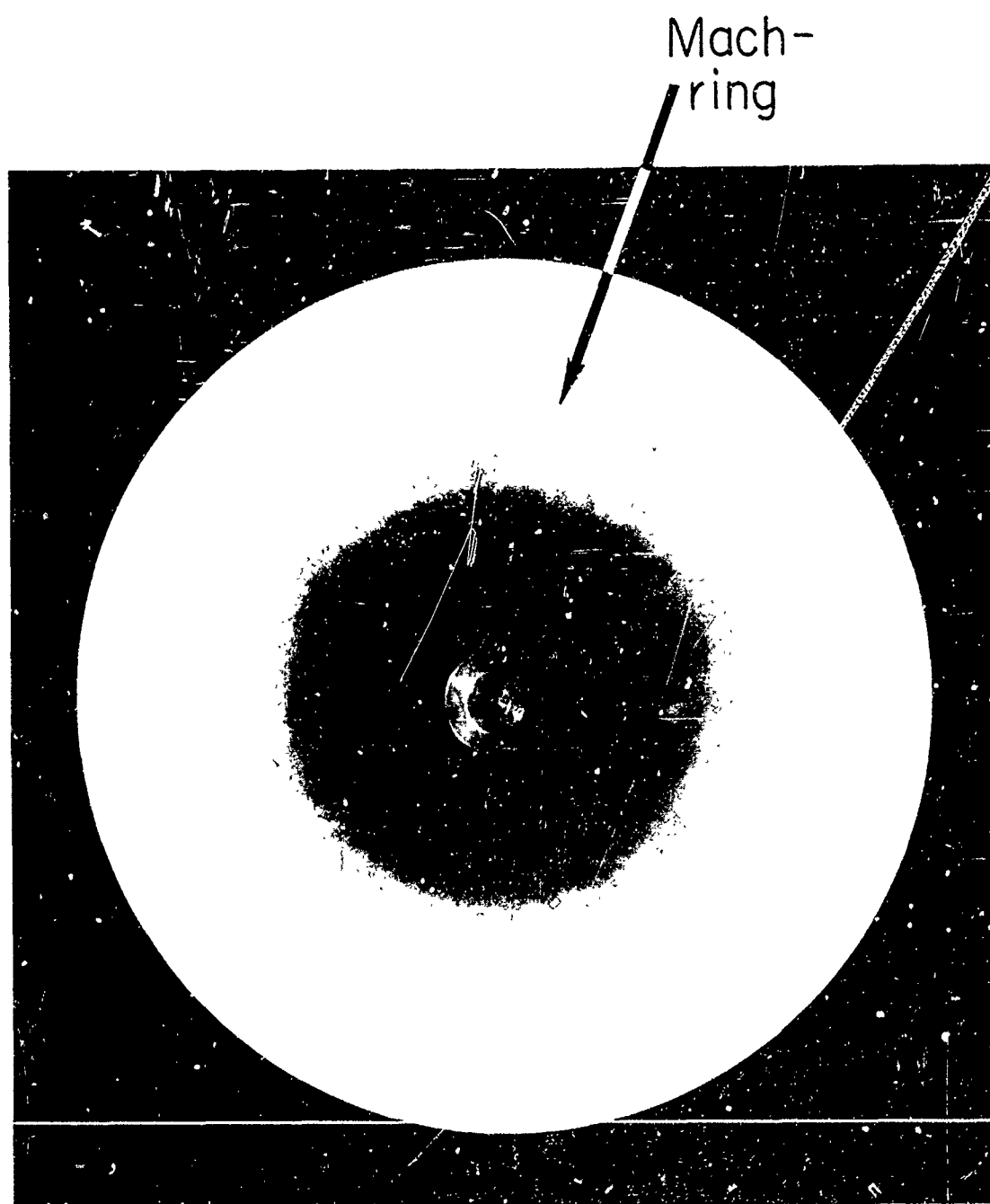


Fig. 4.

medium brightness while at the same time a second, quite bright spot is moved horizontally away from the first spot. If the distance of the first spot from the eye is about 25 cm, and the second spot is moved laterally 50 mm (see Figure 5) a situation is produced in which one of the images of the spots falls on the blind spot of the eye and produces the halo.

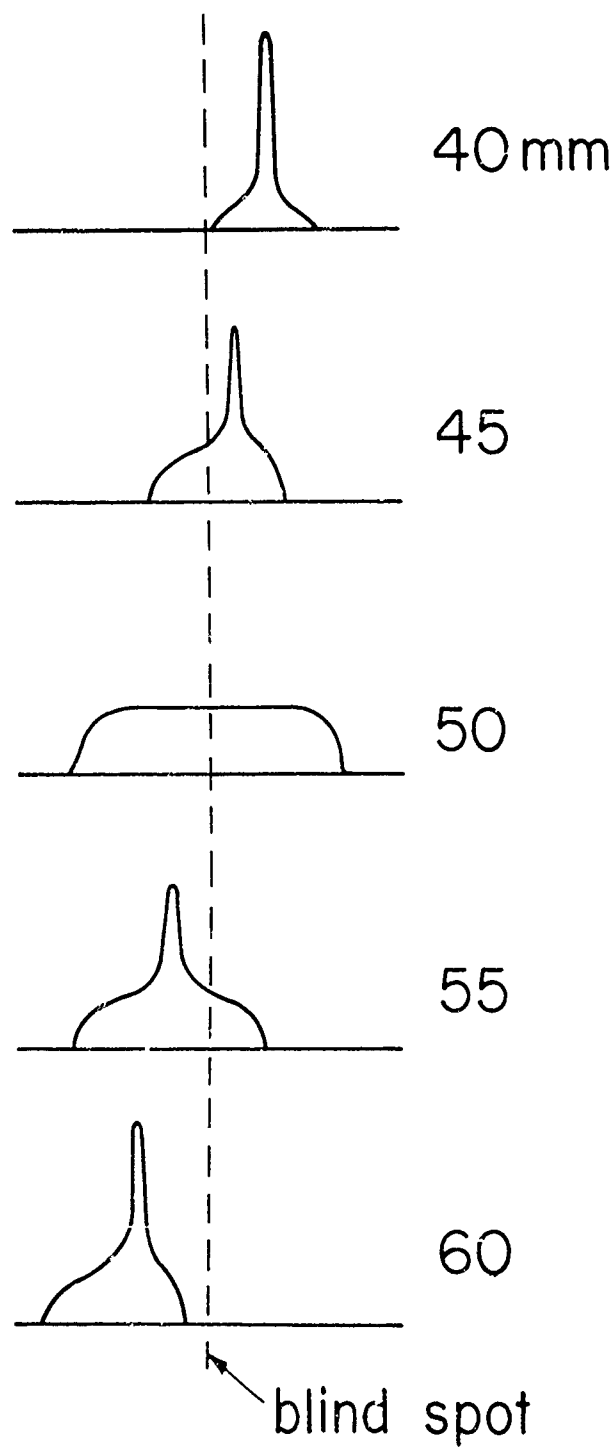


Fig. 5.

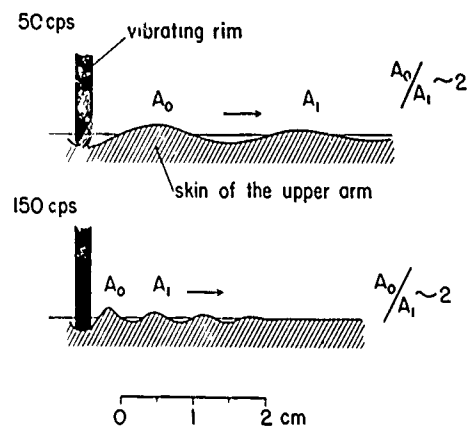


Fig. 6.

Inhibitory effects along the surface of the skin can be very easily demonstrated. In Figure 6 a vibrator is shown touching the surface of the skin, and stroboscopic observation shows a set of traveling waves moving away from the edge of the vibrator along the skin surface. The wave length is long for small vibratory frequencies and short for high frequencies. On the surface of the upper arm for stimulation at 50 cps the wave length might be of the order of 2 cm. These vibrations are damped very little, with the result that the waves may travel around the whole surface of the arm and put every point in motion. In spite of that, only a very small section, whose diameter might be 1 cm immediately below the vibrator, is felt to vibrate. All the waves at a greater distance disappear in the sensation pattern as a consequence of inhibition. From experiments like these it can be concluded that the sensations are localized to the place of maximum vibration amplitude and the place where the stimulus is first presented.

If we generalize from these experiments on the retina and on the skin, we may conclude that every stimulus that falls on a sense organ with large surface area produces, as Figure 7 shows, an area of sensation surrounded by an area of inhibition. I should like to call this inhibited area the refractory area.

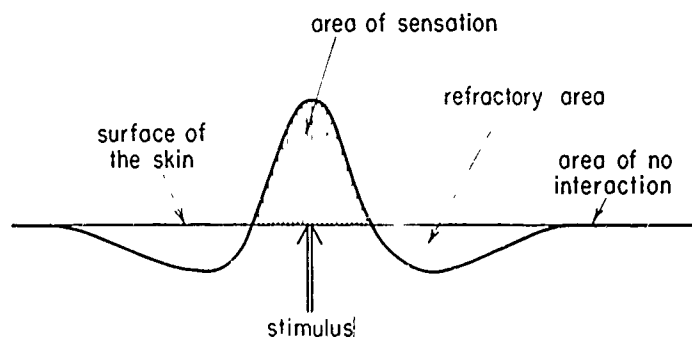


Fig. 7.

In order to illustrate once more the action of this refractory area, we can carry through the experiment shown in Figure 8. Two vibrators consisting of small triangles are placed on the surface of the lower arm. Both these vibrators are adjusted to equal sensation magnitude of vibration. The two vibrators are alternately switched to an oscillator with no time delay, so that when one vibrator is on for $1/4$ sec its neighbor is off.

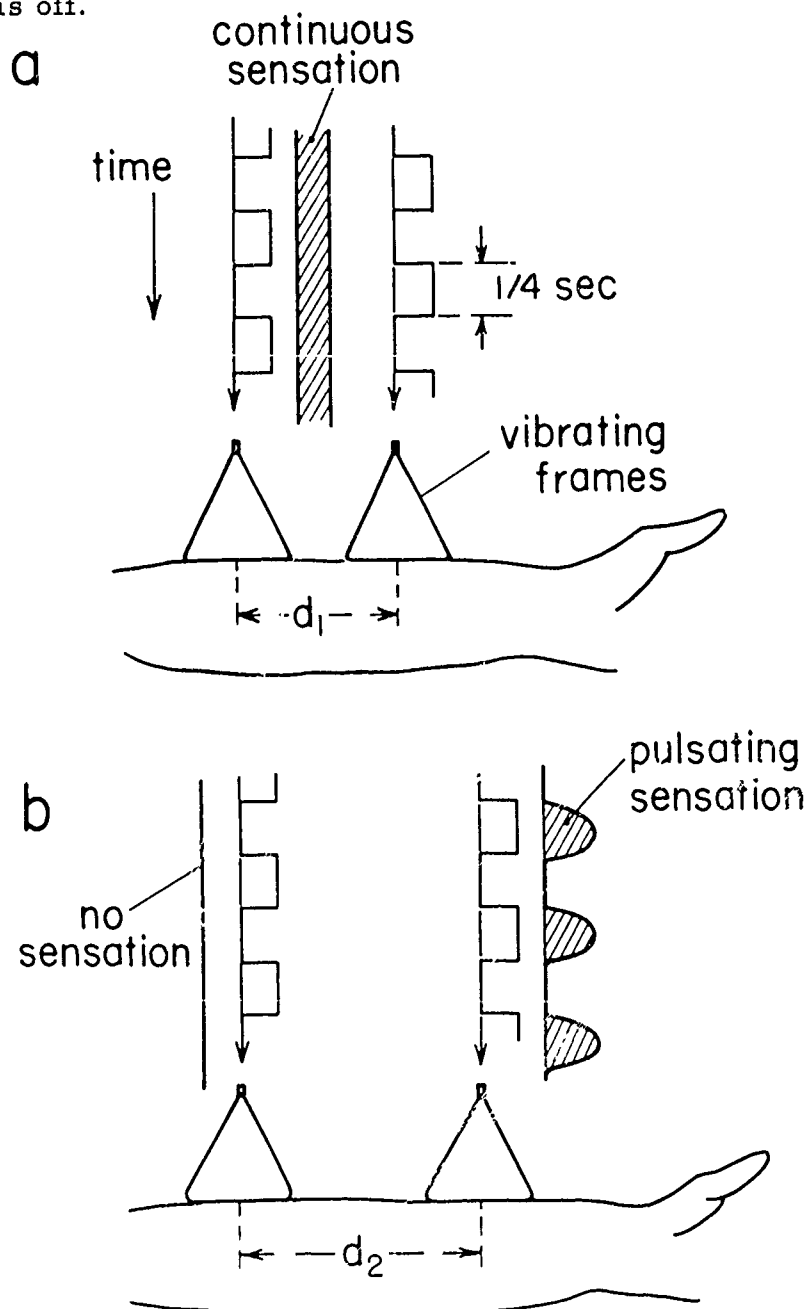


Fig. 8.

If these two vibrators are placed close together along the surface of the skin, we feel a continuous vibration, since the sensations produced by both vibrators seem to fall in the same sensation area (see Figure 7). But if the distance between the two vibrators is increased, a distance may be reached at which one of the vibrators falls in the inhibitory area of the other, and therefore we can no longer add the sensations of the vibrators together. In this case we no longer feel the continuous vibration, but rather an interrupted sensation of pulsating character, as is shown on the right side of Figure 8b.

From these experiments it seems to be possible to represent a stimulus presented to a small area in the form of a unit, as is shown in Figure 9a. In the unit a vertical bar represents the positive sensation and on each side of it there is an inhibitory area. Several experiments suggest that sensation magnitudes and inhibitory effects can,

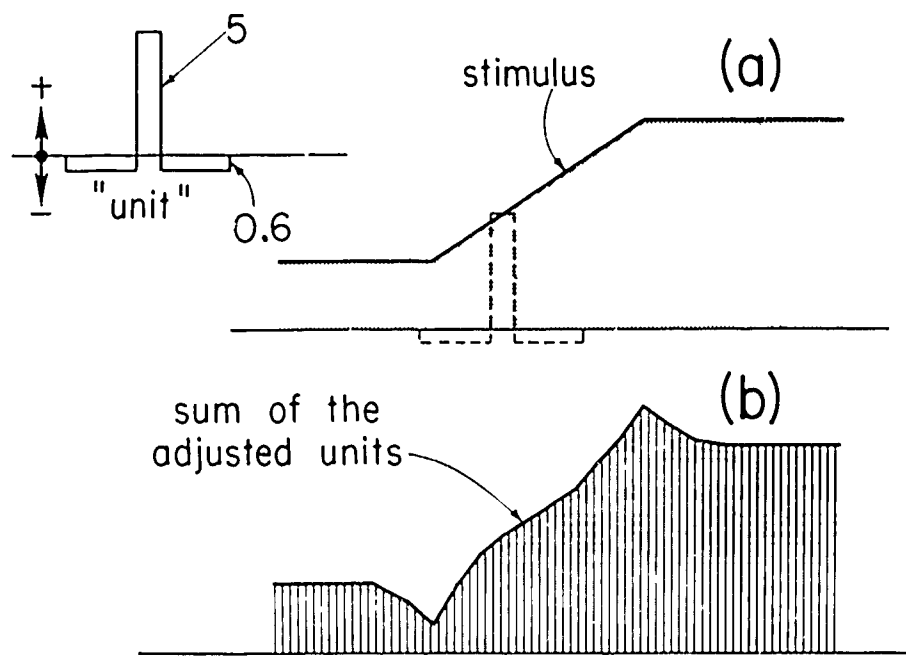


Fig. 9.

initially, be added together and subtracted from each other. As a schematic test, we perform in Figure 9 a summation and subtraction of this type for a stimulus distribution similar to that in the Mach ring, as shown in Figure 3. Figure 9b shows, in agreement with Figure 4, a decrease in the sensation magnitude in the places where the stimulus magnitude suddenly increases, and an increase in the sensation magnitudes accompanying a sudden termination of the increase of the stimulus. Many other situations show similar agreement.

The preceding figures showed how, as a consequence of inhibition, the local magnitude of the sensation is influenced. In the following two figures I should like to show that, not only can the magnitude of the sensation be influenced, but sometimes very pronounced displacements of the locus of the sensations may occur. In Figure 10 a frame was put into horizontal vibration. The frame was attached by four steel tubes to a vibrator. Measurements with condenser microphones made it certain that all points of the frame were vibrating with exactly the same amplitude, and that the amplitudes were not changed by loading the frame with the hand. The hand was placed on the frame in such a way that all four finger tips touched one bar of the frame and the palm touched the opposite bar, as is shown in Figure 10. If we now increase the vibration

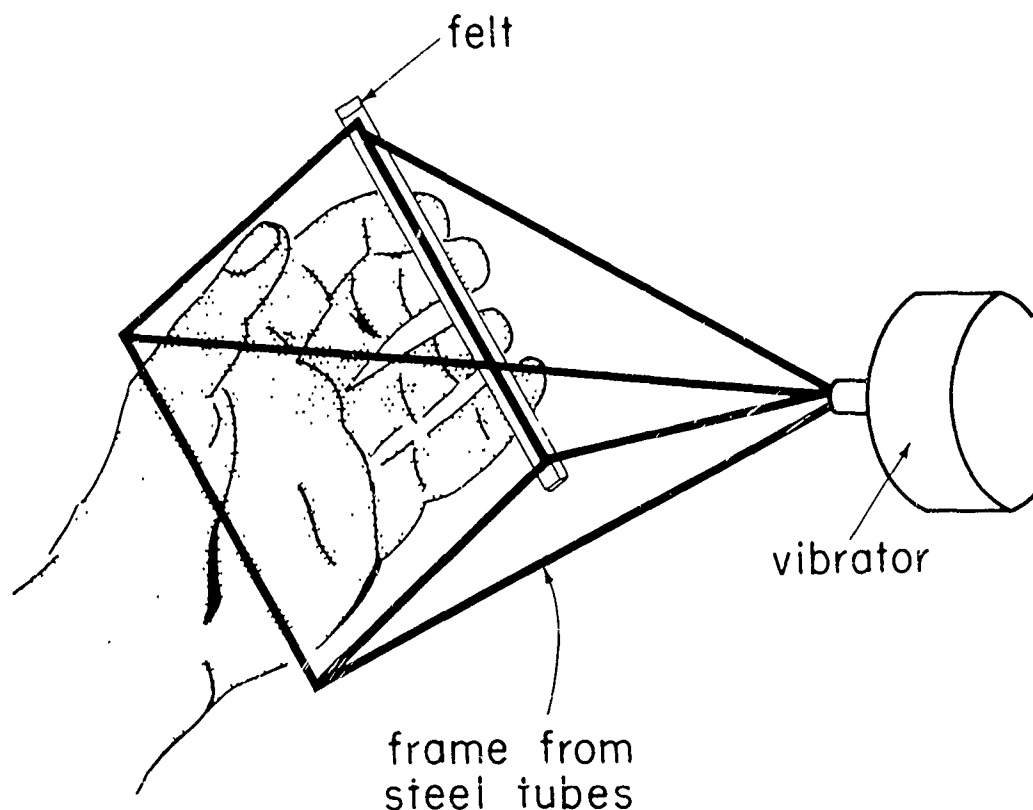


Fig. 10.

amplitude of the frame continuously from zero to higher values, then the threshold of the vibrations is reached first on the finger tips, and we localize the vibrations for low vibration amplitudes on the finger tips. This is shown at the top of Figure 11. As the amplitude of the vibrations is increased we have the sensation that the locus of the vibrations moves from the finger tip to the palm. Therefore the displacement of the sensation locus has been produced by a change in the stimulus magnitude.

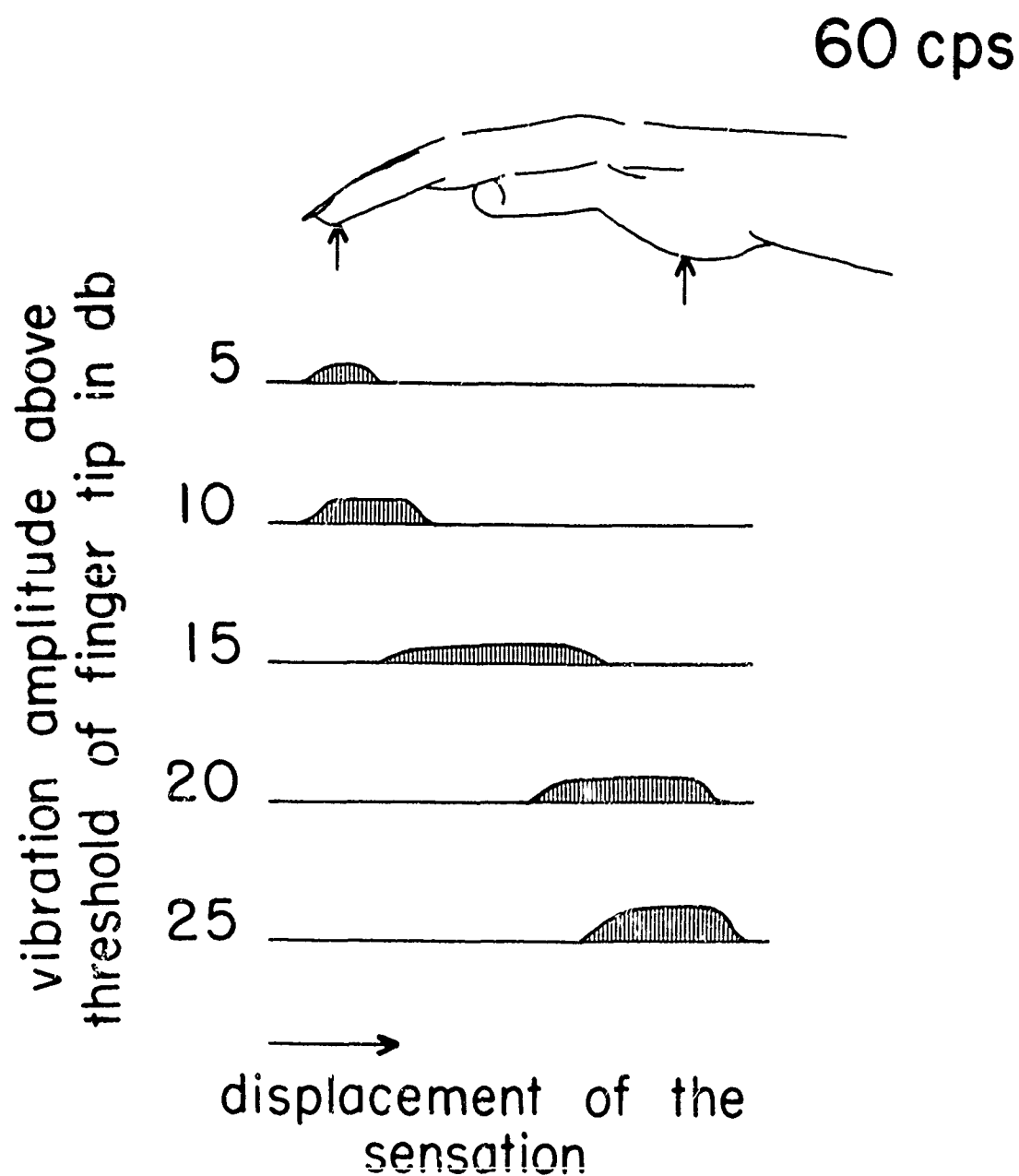


Fig. 11.

This phenomenon is quite clear, but sometimes it is more easily obtained if the relative sensitivity between the finger tips and the palm is adjusted. This can be achieved by putting between the finger tips and the vibrating tube of the frame a thin sheet of felt or foam rubber, as can be seen in Figure 10.

To complete this discussion I should like to mention one other unexpected phenomenon produced by inhibitory effects. This phenomenon

is the "pitch" sensation for sinusoidal vibration. This problem represents, at the present time, a great puzzle for electrophysiological investigations of single neurons. According to all our knowledge of electrophysiology, the sinusoidal stimulus, as represented in Figure 12c produces, near threshold, electric discharges which are in phase

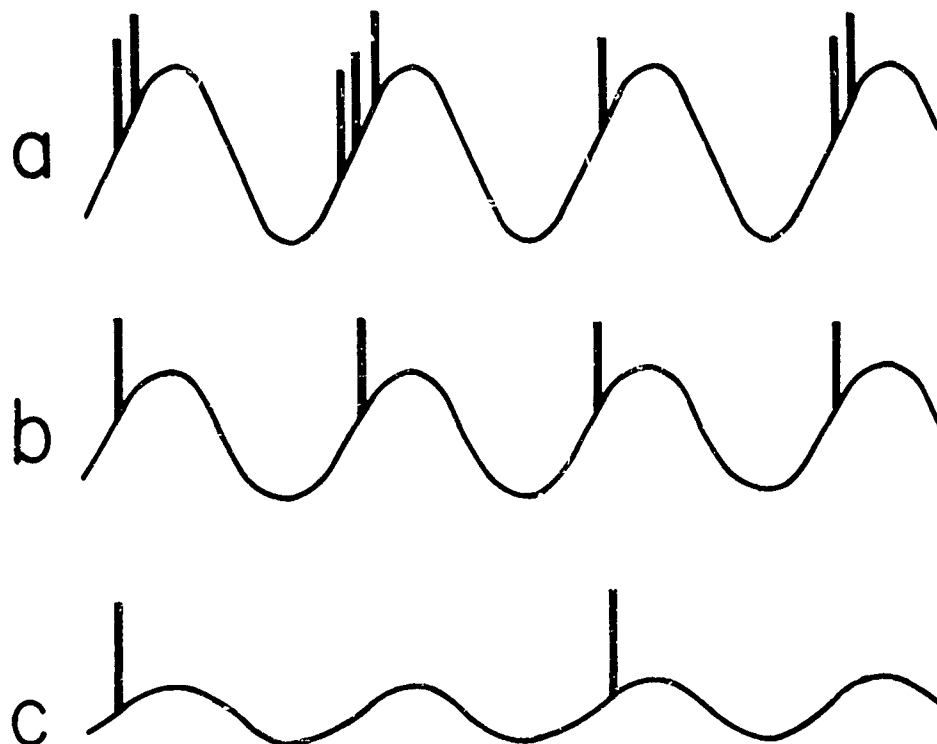


Fig. 12.

with the sinusoidal stimulus; but it may happen, as Figure 12c shows, that a discharge is produced only during every second vibration. As the vibration amplitude comes even closer to the threshold, it may be that even more cycles are necessary to produce a discharge. As can be seen in Figure 12b, at a certain vibration amplitude every single sinusoidal pressure change will produce a discharge. As Figure 12a shows, for quite large vibration amplitudes two or more discharges may occur during one period. From all this we can conclude that the rate of discharge of the nerve elements is low for small vibration amplitudes, and that it increases until there are one or more discharges during every period.

In general, we would like to assume that the pitch sensation of these vibrations on the skin is determined by the rate of discharge of the nerve elements. This assumption would correspond very closely to our thinking along electronic lines, but unfortunately the pitch sensations that

are observed are quite different, as we can see in Figure 13, (3). The pitch sensation of a series of pulses does not increase with an increase in the amplitude, and it may even drop as much as one or two octaves

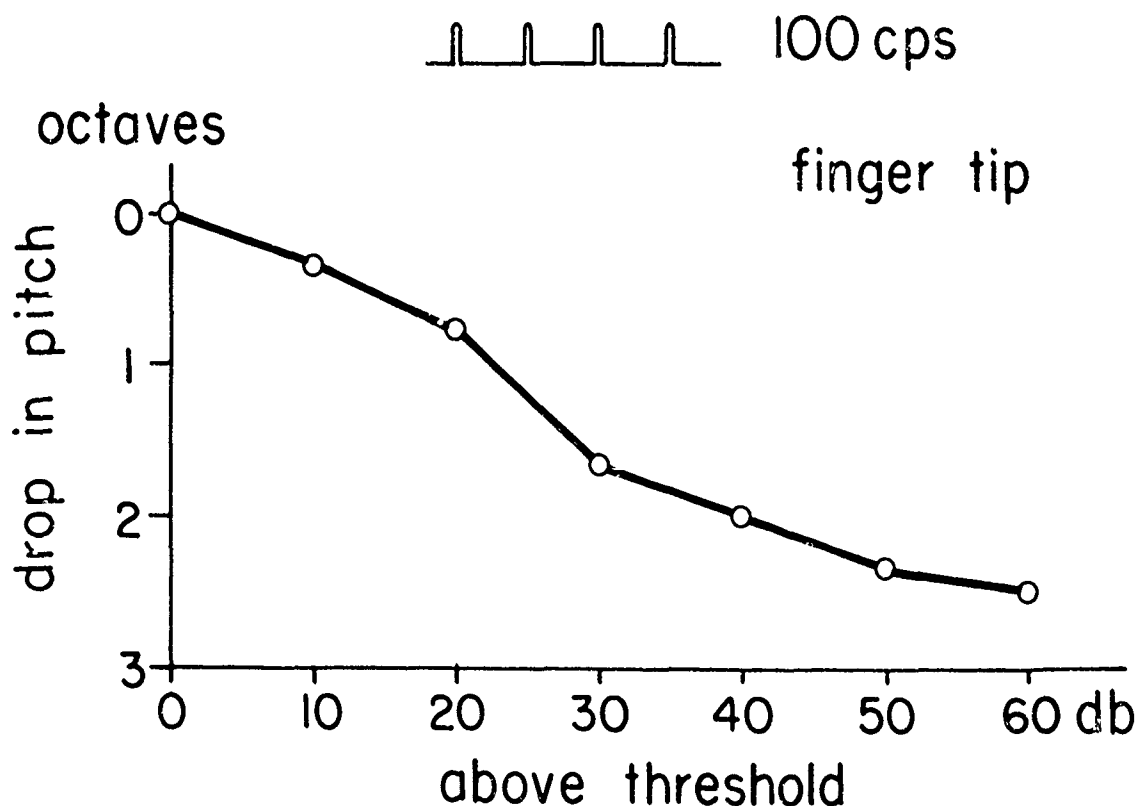


Fig. 13

during an increase in the vibration magnitude. This fact indicates that the pitch sensation as such is not proportional to the firing rate of single nerve elements but rather is a function of some complicated and still unexplained interaction of several nerve elements.

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Some Subjective Magnitude Functions for Touch

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I should like to present some results that are really first steps towards a more complicated goal. In general, we are concerned with quantifying the interactions among the somesthetic senses. Perhaps this is too strong a statement in view of the recent revival of the "Gemeingefühl" (10) and the very recent desertion of the specifists' camp by Hensel (13). What is meant is, "Will concurrent stimulation of a variety ordinarily called cold, or warm, or touch affect the judged magnitude of a stimulus belonging to one of the other varieties of stimulation? In order to do this, one must first find out how each variety of stimulus is judged separately, and to do that he must be able to specify each variety of stimulus in a meaningful way. The importance of knowing the relevant stimulus dimensions, if it needs mentioning here, may be appreciated by a consideration of the difficulties which beset olfactory theory (5), or by the time and energy spent in trying to settle the issue of "temperature gradient" versus "absolute temperature" versus "temperature change" in the case of the temperature senses.

We shall be concerned with one of the somesthetic senses, touch, and the way in which some stimulus parameters affect judgments of magnitude. Since it was our intention to work above threshold and explore as wide a range as possible, the first task was to choose a psychophysical method. Now I have no intention of entering, at this moment, the debate about "discriminability" versus "ratio judgment" as bases for scale construction, except to say that the issue of validity has not yet been satisfactorily faced, and that for our purposes it doesn't have to be. Experience with the method of "magnitude estimation" has convinced me that it is a convenient way of getting a great deal of information in a relatively short time, fairly reliably (4).

In magnitude estimation one sets up a series of stimuli along a sensory dimension, designates one of these by some arbitrary numeral (10 is popular), and asks the subject to assign an appropriate numeral to each stimulus in the series. When the results from several subjects are pooled, it is possible to write an equation relating stimulus magnitude, S , to average numeral assigned, R , which takes the form, $R = aS^b$,

to a first approximation (10). I am aware of no quantifiable dimension of stimulus magnitude reported in the literature which did not yield results of the power function form. This is both intriguing and disquieting. It is intriguing because one might assume that any way of measuring the stimulus that did not yield the expected result would be a poor one. If, for example, I had measured olfactory stimuli in terms of grams of odorous material per 100 grams of solvent (mineral oil), the resulting functions would not have been linear on a log-log plot. Such a way of stating the stimulus magnitude is, as we know, very bad. Unfortunately, some disquietude is also in order, since a pretty diverse set of dimensions has yielded the power function--a point emphasized by Piéron (8). Nevertheless, it was of interest to explore the effect upon magnitude estimates of choosing different stimulus dimensions, with the hope that one dimension might give more promising results than the others.

The choice of stimulus variables is not great. To begin with we decided to vary velocity, load, and depth of stimulus intrusion. Load is, of course, one way of determining depth of intrusion, but it has been used classically, and I wanted to measure depth as a function of load for macroscopic stimuli anyway. An apparatus was devised which would permit manipulation of the variables indicated. Essentially, it is a double-armed balance bearing on one arm a stimulus holder. The other arm is driven upward by a rotating lucite follower which works against a ball-bearing mounted wheel. The follower is driven by a wedge attached to a rack driven in its turn by a pinion connected to a variable speed drive through an electric clutch and brake. It worked with surprising smoothness for lifts of a millimeter and rates of stimulus movement over 100 mm/sec. When load or depth was to be varied, the appropriately loaded balance was allowed to pivot by the unwinding of a thread from a shaft driven at constant speed. The stimulus tip was 2.5 mm in diameter, turned from wood.

Partly on the basis of Nafe and Wagoner's results (6), I rather expected velocity of stimulation to be a very important variable, not only in determining the absolute threshold, but also in determining judged stimulus magnitude. For this reason, a wide range of stimulus velocities was established, the actual rates used being .27, 1.2, 5.7, 26.3, and 120.8 mm/sec. The load series was of necessity more limited in range, the weights being 2.2, 4.9, 10.1, 24, and 50 gm. The deviation from an equal ratio series was occasioned by our dropping out the original lowest weight because of too many zero judgments. Depths were set on a simple linear scale for ease of control, and were 1, 2, 3, 4, and 5 mm.

In the cases of load and depth, the stimulus was applied at the rate of 1.9 mm/sec. In each series, the middle value was our arbitrary standard, and called 10.

In all, 24 Ss, all undergraduate, were run through these series. Six are not represented in the load data because our series was changed for the seventh and subsequent Ss. The usual method of magnitude estimation was used except that the standard was given twice before each series and not subsequently repeated. Each S judged each stimulus twice.

Figure 1 is a log-log plot of the data for velocity. Each data point is the median of the individual averages; the median gives results in this case not too far from the logically correct geometric mean. The power function fitted to these points by least squares is $R = 7.02 S^{.13}$. The obtained exponent is very small. This is especially interesting since the more obvious artifactual stimuli--noise from the apparatus and possible after-vibrations--were such as to increase the likelihood of larger judgments for the faster stimuli. Despite this, the faster stimuli are judged but little more intense.

FIGURE 1

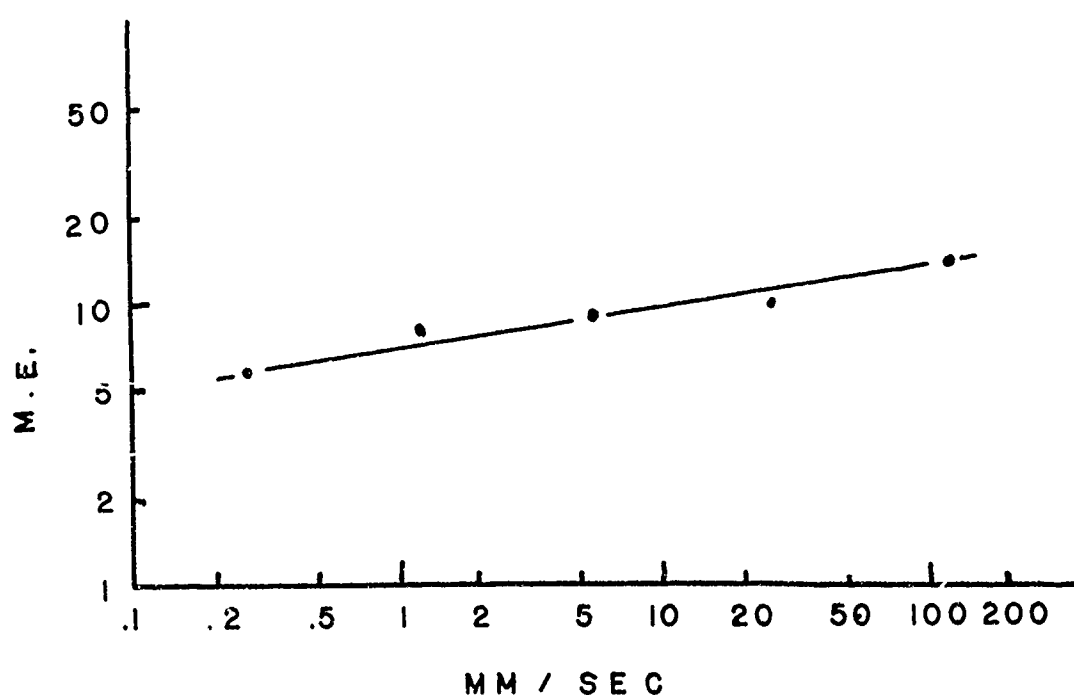


Fig. 1.

Figure 2 shows the results from a check experiment using a .5 mm movement. Because of the design of the apparatus, rates are also one-half those of the previous series. The number of Ss, five, was smaller, but ten instead of two judgments were obtained from each. The power function fitting the data is $R = 5.13S^{.34}$. The somewhat larger exponent is of possible interest, although I am inclined to reserve interpretation until more data can be collected.

FIGURE 2

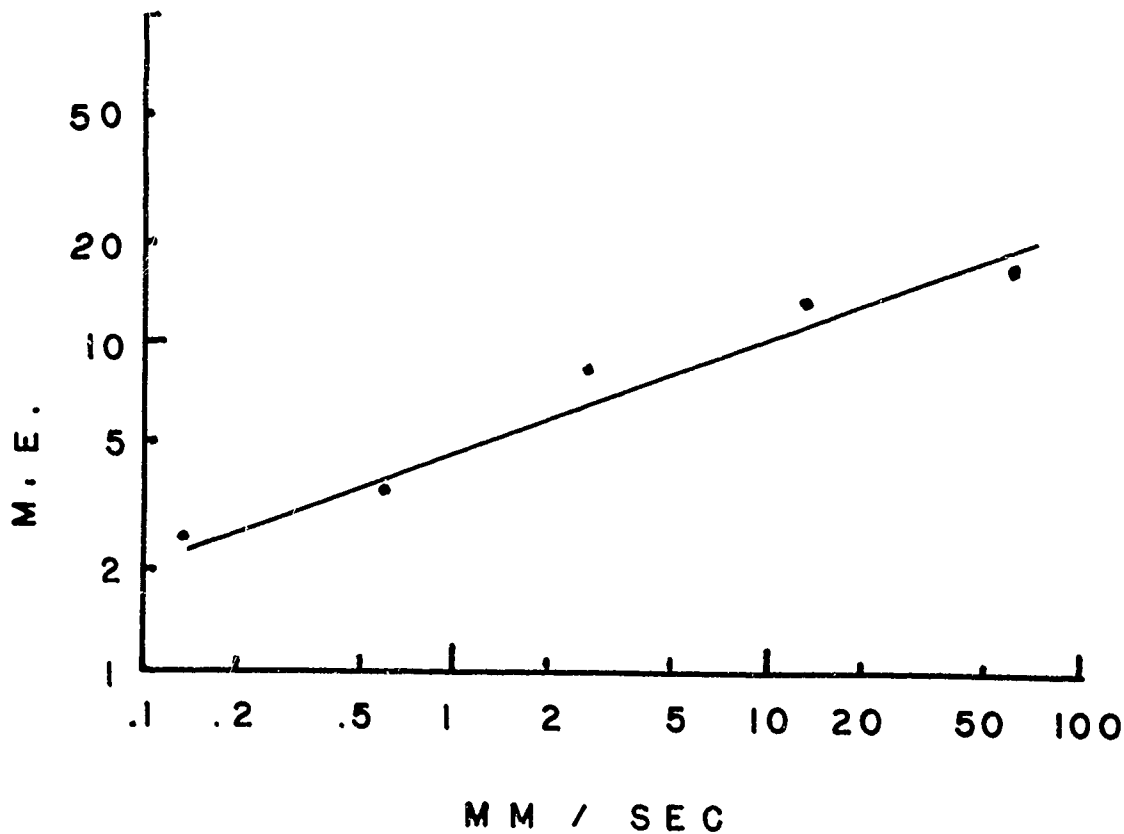


Fig. 2.

Figure 3 shows the depth data. A linear plot would have done as well, since the fitted power function is $R = 3.89S^{1.02}$. Over the range studied, S judges the intensity of pressure stimuli precisely according to the total linear displacement of the skin. This being the case, it seemed probable that differential loading could best be described in terms of displacement. Figure 4 shows the measured skin displacement for several Ss as a function of load. It will be seen that this is a log-log plot, and is fitted by the function $D = .33L^{.6}$, where D is displacement in millimeters and L is load in grams.

FIGURE 3

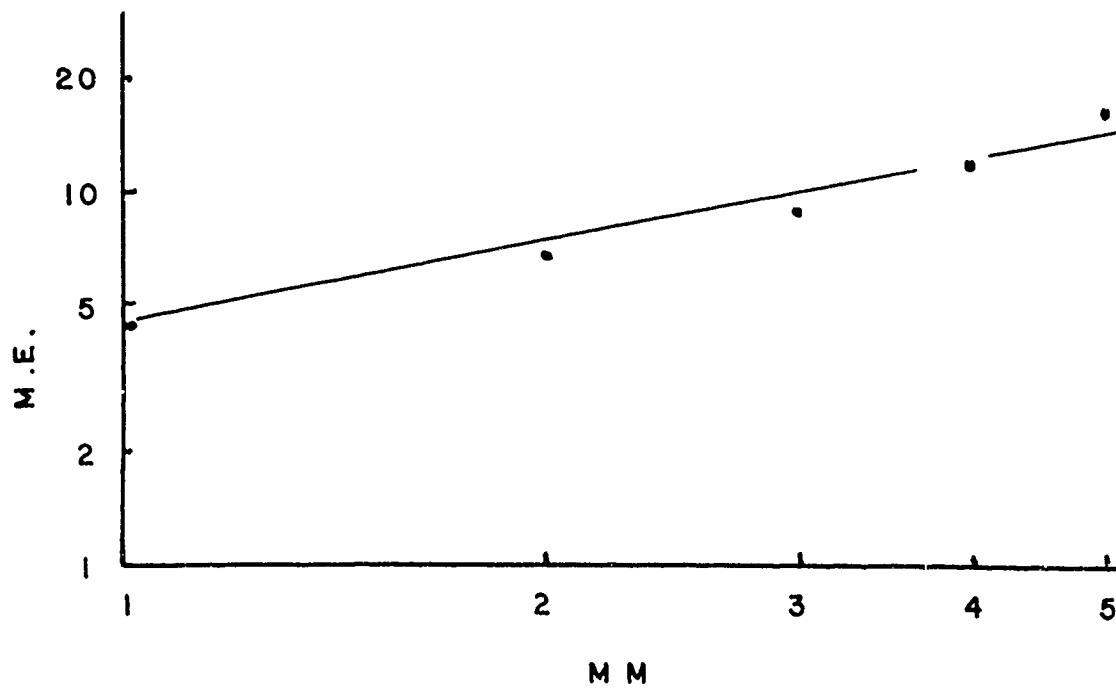


FIGURE 4

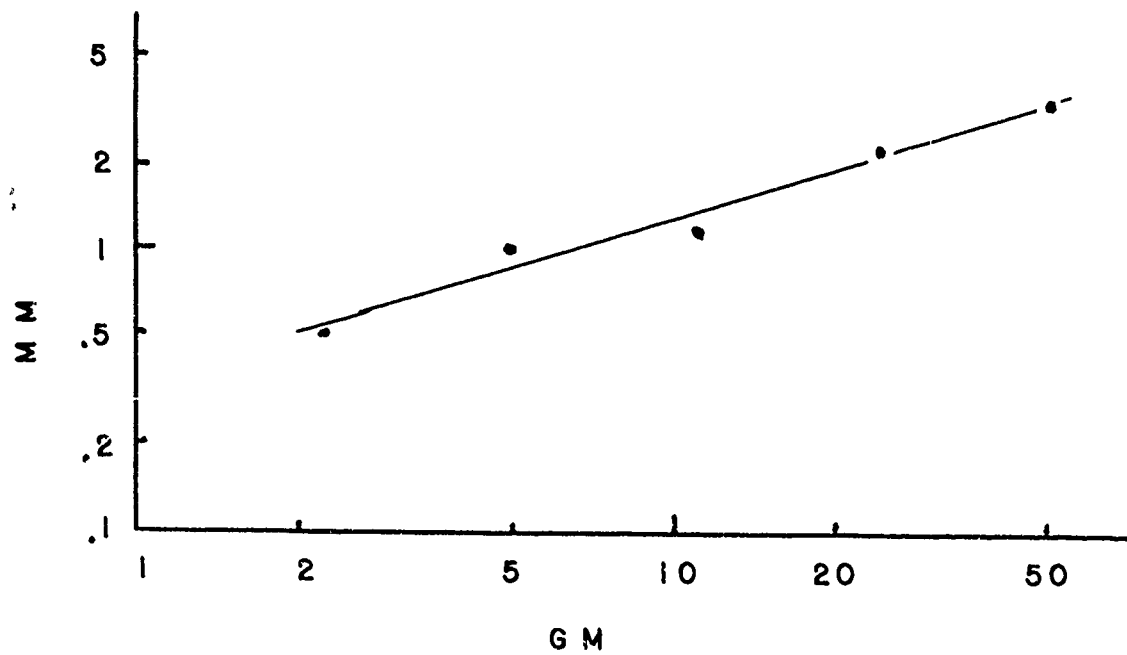


Fig. 3 and 4.

Gatti (1) gives $D = K\sqrt[3]{p^2}$ which is very close. After transformation to depth the load data look as in Figure 5. The power function here is $R = 7.4S \cdot 74$. The number of Ss here is a little smaller, and an increased N might bring the exponent closer to the depth data.

FIGURE 5

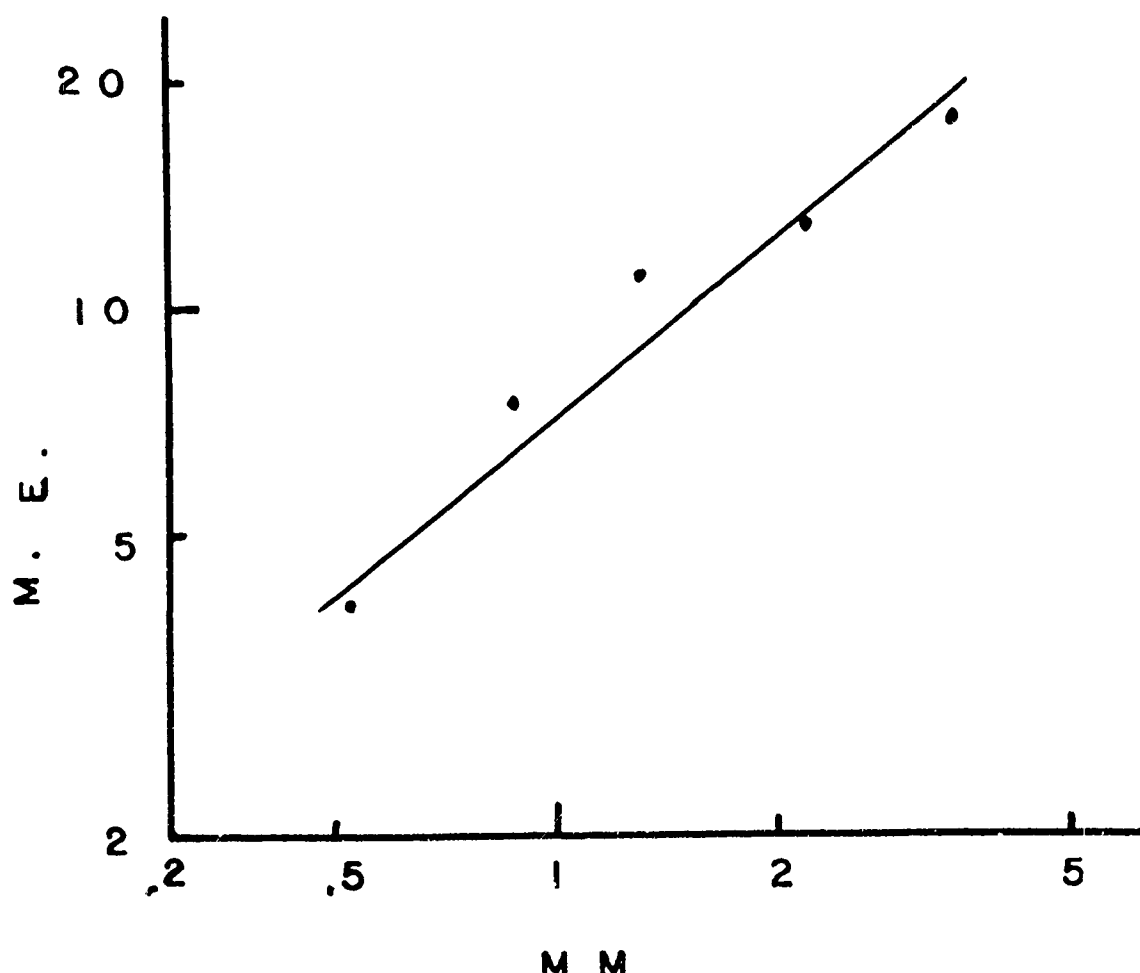


Fig. 5.

Since all the above data refer to a fairly large stimulus tip, I was curious to find out what would happen if we used a small tip. Therefore, we constructed a graded series of hairs .2 mm in diameter. Bending loads were .2, .36, .65, 1.17, and 2.10 gm. We used six Ss, ten trials each. Since the diameters were the same, a transformation of the von Frey variety, gm/mm, would make no difference to the form of the results (11). Load plotted directly against judged magnitude did not yield a straight line on a log-log plot, and it was assumed that our Ss

had skin similar to Gatti's and grams were transformed to millimeters. The transformed data are shown in Figure 6. The fitted equation is $R = 1.03S^{1.3}$, which is not too different from the macroscopic data, considering the transformations involved.

FIGURE 6

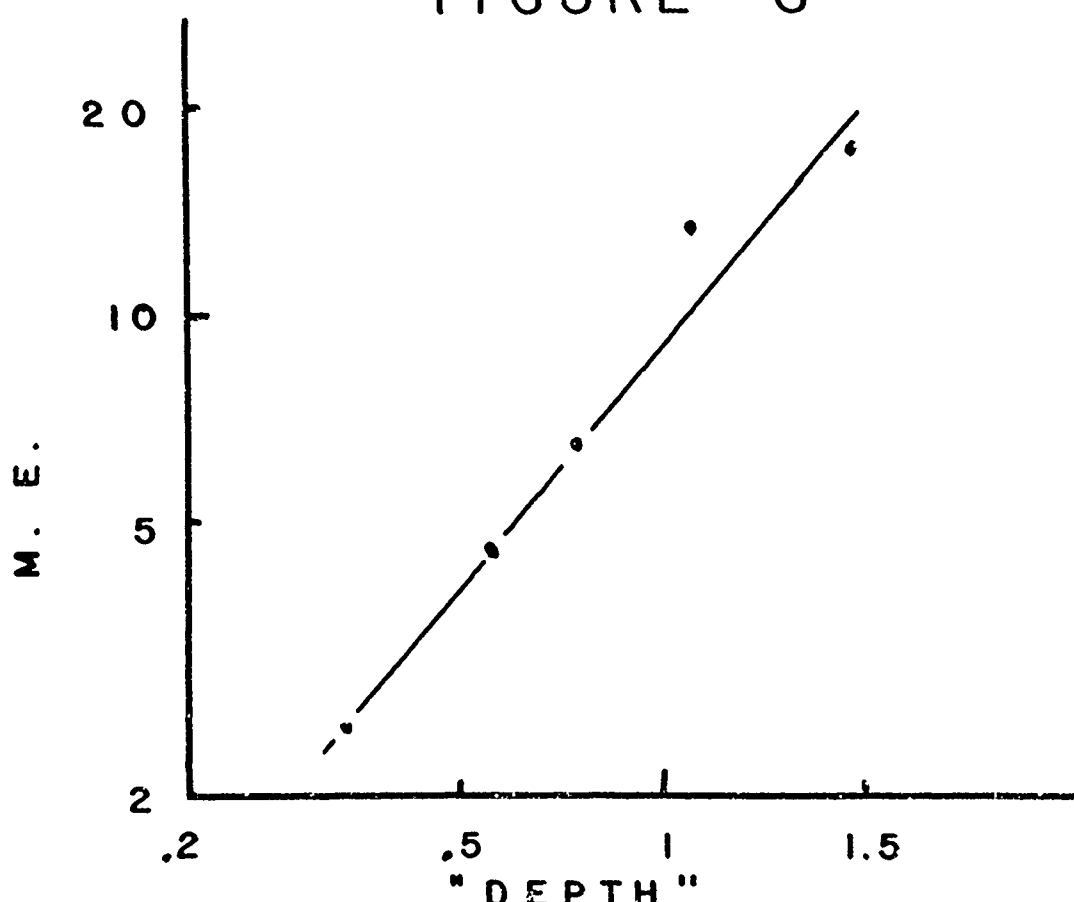


Fig. 6.

It is always tempting to speculate about data such as these. It is especially tempting to construct post hoc neurophysiological explanations. But our concern is with the total psychophysical process, not simply the events taking place at or near the receptor. If we are permitted a systems analysis analogy, it may be said that our experiments aim toward the discovery, or construction, of system parameters. It is not possible to specify system properties by observing simply the input and output of a system. This does not mean, of course, that input-output studies are useless. On the contrary. So many levels and varieties of transformation must exist that the delineation of the system properties of the organism and especially the prediction of parameters

therefrom are of extreme difficulty. Nevertheless, the results of research upon the properties directly must be compatible with total system characteristics, even though the statement of the relationships may be very general and quantitative only to a rough approximation.

An important consideration must be the effect of past experience. It is quite possible, for example, that continued experience with different rates might lead to an expansion of the subjective scale. I have suggested elsewhere (3) that we are dealing in experiments of this kind with a variety of perceptual constancy based upon learning, and Warren (12) has argued at length for a similar idea. Needless to say, at least the numerical categories must have been learned, and I have shown elsewhere that the individual S seems to take his own conception of the number system with him from modality to modality (4). So the firm conclusion from our results so far is that for our future purposes varying rate is not the stimulus parameter of choice. One gets too little change in judgment for so much change in rate.

Despite the logical hazards, it is of interest to ask whether or not the scaling results have any implications about the action of the touch system. Certainly the results are compatible with the idea that touch receptors adapt very rapidly, or at least that the system skin-plus-receptors has a very short time-constant following a shear stress. Since this is the case, the relatively small effect of rate of stimulation makes necessary the postulation of some kind of integrating process extending over at least 3.7 seconds, or, alternatively, the suggestion that frequency of neural discharge in a given afferent fiber is not as important as the number of fibers active (cf. Uttal's paper in this symposium). The depth data suggest much the same thing, with the added possibility that which receptor is stimulated is also important, since the deeper stimuli must affect receptors not aroused by the shallower intrusions. The situation is analogous to that in audition, where recruitment implies that some receptors give rise to activity judged intense in and of itself. There remains the possibility that the more intense stimuli stretch the skin and change fast adapting receptors to slow ones, as occurred in the frog skins studied by Loewenstein (5).

Of especial interest, in the light of Iggo's results (2), is the question of the role played by the "C" fibers. Apparently, most fibers responding to a touch stimulus, in the cat, at least, are "C" fibers. The associated receptor processes seem to be rather slow to adapt. If activity in these fibers is involved in judgments of intensity, the relatively small effect of rate of stimulus application to a constant depth is readily understandable. Unfortunately, there is no good evidence that skin "C" fiber

activity ever reaches the cortical level. More research is needed here, as in all the other problems I have so briefly mentioned in these paragraphs. In the meantime, even though we do not understand how the system works in detail, we have some beginnings of statements concerning its overall parameters.

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Discussion of the Preceding Four Papers

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The papers, and parts of papers, bearing upon basic theory of the somesthetic sense, which I have been asked to discuss, and I include parts of Dr. Hahn's paper, will be treated under four headings, the first three dealing with common pressure sensations: The first topic is the encapsulated "endings" found in the skin, as end-organs selective for qualitatively different stimuli. The second, adequate stimulation for pressure sensations. The third, adaptation to pressure stimuli, and the fourth, the temperature sense.

1. The evidence cited makes it quite impossible to retain the theory that the capsules are end-organs, each specific for a single quality of sensation. None of them have been found in any of the hairy parts of the body, which are well known to be sensitive, generally, to pressure, warmth, heat, cold and pain, and that one fact is conclusive. The proposed correlation was a mere suggestion on the part of v. Frey, Goldscheider, and others and in a century of investigation all attempts to demonstrate such a relationship have failed. In the papers presented here Dr. Jones cites Hensel, and Dr. Uttal cites Weddell, as having abandoned the theory. Dr. Hahn also comments on it and all three of our speakers imply, or state, that they too have abandoned it. Perhaps, at last, it is on its way out.

This leaves only free nerve endings to do the job and, beside differences in degree of myelination, these differ in only three ways: 1) in diameter, hence in the functions of irritability and speed of conduction; 2) in the manner of peripheral termination; and 3) in their central connections. In general they function alike and this forces a theory of patterning of nerve discharges, temporally and spatially, to correspond with the qualitative variations of the aroused sensations. Such a theory was posited, and evidence adduced to support it, by Nafe and Wagoner more than a quarter of a century ago.

2. The evidence cited by Dr. Kenshalo as to movement as adequate stimulation for pressure sensations is convincing. In all the years that these researches have been going on, by physiologists and

psychologists, he reports the first monitoring of the stimulus. Adrian noted that the hair gives rise to sensation only while moving--he called it "dead beat"--but even he did not follow it up in other experiments on pressure sensation. Adrian also noted that adequate stimulation of plain muscle is tension and not distension, and the records shown by Dr. Kenshalo are a good example of what is meant: "the stimulus" is still present, the tissues are still distended, but the tensions have been resolved and adequate movement and nerve discharge have ceased. In the records shown nerve discharge begins with movement of tissue and ends when such movement falls below a minimum amount or velocity or both. Such adequate stimulation often is called "tension" but the meaning is the same because tension in pliable tissue means movement toward an equilibrium condition. Many investigators have concluded that except for electrical stimulation and injury, tension, or movement, is the only adequate stimulus for pressure sensations. The series of possible events between the movement and discharge of the nerve fibers are not of importance here. Also the relative importance of amount and velocity of movement under different circumstances is not yet well defined. Both Drs. Jones and Uttal have reported here that, under certain conditions, velocity of movement played a small part, or no part at all, in their results. Yet the records shown by Dr. Kenshalo clearly indicate that when depressed tissue is released there is as much movement back to its normal position as there was during depression, but much slower. The nerve discharge during the return is much less in frequency and duration than in the response to the quicker depressing, and with human subjects one gets reports of much less intense sensations.

3. Adaptation: It follows from Dr. Kenshalo's records of pressure stimulation that the failure of nerve discharge with "the stimulus" still present, usually called "adaptation" or "accommodation," is not due to some property of, or change in, the nerve fibers, e. g., fatigue, but refers only to failure of stimulation. This is a very important finding and provides another basis for comparing the phenomena of other sense departments with that of somesthesia, as both Dr. v. Békésy and Hahn have done. Both of these presentations are very important, not only for the immediate demonstrations, but for the suggestions as to work to be done. Neither paper needs explication or enlargement by me at this time.

4. Temperature sensitivity: The experiments reported by Dr. Kenshalo were suggested in part by just such considerations as those we have been discussing. With the demonstration as to the nature of

stimulation and adaptation for pressure sensations, we turned back to the earlier work on temperature sensitivity and especially to the origin of some of the persisting ideas or theories of such sensitivity. It seems to me that explanation of somesthetic sensitivity in general was based upon analogy with the Young-Helmholtz component theory of color vision. In fact, that seems to have set the stage for theories in all the other sense departments. Not one of them has been demonstrated to be true, including that for color-vision. Perhaps progress would have been greater and faster had we likened some of these processes to those of hunger, thirst, nausea, and the biological urges. These well defined feelings usually are correlated with temporal and spatial patterns of nerve discharges aroused by tissue movement, e. g., of the sphincters, or with changes in tissue which result in modifying responses to usual movements, such as the dehydrated tissues in thirst.

The reaction of blood vessels, and other smooth muscle, to changes in temperature have been in the physiological literature for many years. Also the sensory effects of temperature changes have been in the psychological literature for many years. Many years ago attention was called to the parallel between the two. Many years ago Goldscheider, following his excision experiments, identified the peripheral arterioles as the constant correlate of warmth and cold points on the skin. We all know how difficult it is to see what we are not looking for and don't know about. Investigators were looking for modified nerve fibers and although it occurred to some of them that the vessel walls might constitute a satisfactory substitute, the thought was abandoned because they did not know of the local response of smooth muscle to temperature changes and they thought only in terms of reflex reponses when some specific end-organs were stimulated.

There is no claim here that the last word has been said, but there can be no reasonable doubt but that the patterns of nerve impulses aroused by vascular responses to temperature changes, and perhaps by responses of the erector muscles of the hairs, are the neural analogues of temperature sensations.

Other parts of these papers are of interest and importance. It is through such studies that this sense department will be defined and understood. I cannot at this time enlarge upon or clarify them although the estimates of sensory magnitude is of general interest for theory as well as being of importance for communication through vibratory, pressure, or electrical stimulation of the skin. Nothing has been said in the papers this morning of the essential nature of somesthetic sensitivity to vibration but Dr. Geldard's findings apparently settled that matter some years ago.

Possibilities of Cutaneous Electro-Pulse Communication*

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The dilemma of the work in the field of vibro-tactile sensitivity has been summarized by Geldard with the statement that even "applied" problems are in themselves "basic" because so little is really known about the skin as a sense channel (1). I shall not discuss the history of cutaneous sensitivity. It could be written by anyone in this august group of pallesthesiologists; so I shall confine myself to an applied problem which is basic in every dimension. In fact, this problem is so new on the horizon of cutaneous communication that almost its entire history is with us at this conference in the persons of Doctors von Békésy, Hahn, and Uttal.

When one considers some non-auditory and non-visual communication problem in a military setting (such as talking to tank drivers, frogmen, and astronauts) one must face several different sets of questions: What are the demands of the communication system in terms of information to be conveyed? Is the skin capable of transmitting information accurately, and under what conditions? Can cutaneous signals be coded to give both context and content to a language? Is this form of communication practical for complex information processing?

The possibilities of making use of the skin as another sensory communication channel have captured the imagination of a few investigators from time to time since the early basic works of von Frey (14) and the writings of Nafe (11). In the past three decades several attempts were made to circumvent deafness through leading speech sounds into electro-mechanical transducers applied to the skin. In 1957 the first successful attempt in tactile literacy was reported (1, 10). The recent works of the Virginia group, von Békésy, and of others in this conference have opened up the basic problem areas of "asking the skin what language it could compass." We now know that mechanical vibro-tactile communication under laboratory conditions is feasible. In a recent issue of Science Geldard has presented a summary picture of some neglected possibilities of cutaneous communication (2).

Why should attention be given to building a communication system based on electrical impulse signals, particularly since so little is known about electro-anatomy? We have little or no knowledge about what kind of changes electrical inputs into the skin induce under conditions of mild

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stimulation. Gilmer and Haythorne (4, 5) have shown that excessive stimulation of the skin with sinusoidal current can, under certain conditions, destroy epidermal and dermal tissue. What are the limits between "mild" and "excessive" stimulation that may be of importance in practical situations? If it proves possible to stimulate the skin electrically with very low frequency pulses (1 - 8 p.p.s.), where tissue is not injured and pain is avoided, then electro-pulses have the advantage over the use of mechanical vibrations in that they can be defined in their quality and confined to the immediate environment of the active electrode. This is apparently in contrast to the diffuse spreading out of mechanical stimulations. There is another reason for our choice of using electro-pulse as our cutaneous signals which may be more defensible -- like the mountain, we are interested in it because it is there. Some of the recent work of Hahn (7) and of Gibson (3) indicates that it may be possible to stimulate the skin electrically under conditions where pain is avoided. The work of Uttall (12) has given us encouragement to proceed in this direction.

We believe that it is possible through a cutaneous communication system, to provide information of such diversification as stock market quotations, weather forecasts, and gun laying data. One widely applicable area includes improving safety through redundancy. From "vibratory barographs" for use by frogmen to "covert codes" for use in confidential transmissions, vibro-or electro-tactile communication problems fall into a minimum of seven classes of information that at the present time can possibly be conveyed through the skin. Through "amounts" one can present quantitative information, and through "coordinates" one can give relational information. In landing an air-craft "directions" and "rates" can be transmitted to the skin. It has been shown that through the use of vibratase one has the potential of a practical "language" system superior to Morse Code (1). The attention-demanding qualities of vibration give it some uniqueness in "warning". Just where this mode of communication may fit into "vigilance" problems is not as yet quite clear.

One important research area lies in comparing and contrasting tactile sense channels with hearing and vision. Henneman (9) has made comparisons between the eye and the ear in communication. The eye is best as spatial sense, the ear holds superiority in the temporal dimension. There is some evidence from recent works in the Virginia Laboratories that the skin may be unique in combining the spatial-temporal dimensions into one sensory channel. One possible new research area here lies in determining just how vibration may be intertwined with vision and hearing in order to maximize sensory reception, provide for redundancy where needed, and serve in some manner or other in reinforcement.

In terms of considering "vibration" (or "electro-pulses") as a communication channel, several advantages have either been demonstrated or come to mind. A sample of these follows. Vibratory stimulation can be used: (1) as an aid to spatial orientation, (2) in making relational

comparisons, (3) to quickly alert or warn, (4) where there is demand for rapid referability, (5) where unusual stimulation is desirable, (6) as aid to vigilance through both warning and redundancy, (7) where uniqueness of situation can make vibration a part of the habit structure, (8) where environmental conditions handicap both auditory and visual presentation, (9) to supplement communication in multiple task performance, (10) where previous habits are not auditory or visual, (11) where response quickly follows presentation, (12) where simple reference information is needed continuously over long periods of time, (13) for simple signals anticipated by operator, and (14) where conditions handicap eye and ear.

Much work has been done in identifying the nature of the stimulus dimensions for mechanical vibratory stimulation. Duration, intensity, and loci have been built into a successful vibratese language. The limitations of using frequency have been analyzed. The secondary dimensions of on-set, numerosity, and interval between stimuli may well offer additional avenues for discrimination. Stimulation electrically with pulses, and possibly sinusoidally, built on such basic work as that of Hawkes (8), appears to have promise in providing a manageable system for stimulating the skin. Not only does electrical stimulation hold possibilities as analogs to mechanical stimulation, but it has some unique properties of its own at the qualitative level. Work now going on in the general exploration of the skin with electrical stimulation holds promise of opening up a whole new area of problems, both psychophysical and applied.

Coding, in contrast to the many studies on dimensions, has had little work done on it. One controversial point lies in the approach. Should attempts be made to build up a general skin language or should coding be made specific to each problem situation? Shannon has estimated the redundancy of English based on the statistical structure of the language over distances of about eight letters to be 50%. Important in this problem area is the computation of the capacity of the observer in receiving information per unit of time. How many bits of information can be transmitted by the skin under some given condition? In commenting on some studies involved in the air defense network Gregg (6) concludes that "engineering the language instead of the machines has proved to be an effective way of overcoming the difficulties in the communication system." Intelligibility can be stepped up by increasing the redundancy of the message by adding words or phrases or simply repeating the entire message a second time. More important for our problem of coding is the fact that the same effect has been obtained through the hearing channel by reducing the number of allowable words or symbols. In one situation involving an analysis of the content of the messages, it was found that about five bits of information could be used to provide a satisfactory language that led to almost 100% accuracy in the transmission of speech in what was actually a very poor system. One may, therefore, postulate that by its very nature the skin is not handicapped with a lot of excess verbiage as are the written and spoken word. Perhaps the skin has the possibilities of codes even superior to other channels because of its "simplicity."

Knowledge of the stimulus variables in tactile sensitivity is practically non-existent. Aside from acuity relationships at the finger tips, the results from only a few studies on adaptation, and some meager information about masking, little is known. The few conclusions from work on apparent movement indicate that this may well be an avenue for exploration into the uniqueness of the skin as a spatial-temporal receptor. Any one of these variables offers new research possibilities. Individual differences in receptivity to electrical and mechanical stimulation certainly exist. Before any practical communication system is installed in a military, or other situation, we will have to have measures of "skin deafness" as well as knowledge about channel loads, the effects of distraction and error ranges.

Important to a cutaneous communication system are questions of stimulus presentations. Whether the stimuli should be presented as discrete elements, regular or irregular, in sequence or not, for a fast response or for a slow response, depends largely on the demands of the output.

The information loop, so common in conventional forms of communications, such as in the completion of a telephone linkage from caller to receiver back to caller, may well have some potentialities in the tactile area. One example is found in the practical problem of the pilot who gets a vibratory signal on his foot warning him to ease up on the brake pedal. The easing off causes a cessation of the signal and hence the loop has been completed. This is an area for development rather than research.

There are two kinds of warning situations favorable to vibro-tactile stimulation. One involves the "break in" to an on-going activity where attention is demanded above and beyond the activity in which the receiver is engaged. Research in this area should center around the proper signals to use on the proper skin loci. Here one might well wish to compare vibration with the other sense modalities for, say, speed of reaction. The second type of warning may be described as "drawn out," a pre-warning situation as it were. This is exemplified in the case of a person who loses vigilance on the job that does not demand near constant attention. Thus, a warning that attention may soon be demanded can ready the individual for the situation.

The skin as a sensory channel may have one completely unique aspect; namely, it is rarely ever "busy." True, it receives bumps, adapts to the environment, but for the most part it is not used as frequently as the eye and ear. This provides opportunity for it to learn, to become habituated to code that cannot be interfered with under certain conditions. The basic problems in this area are concerned with the capacity of this channel to handle information, when and where to use the channel most effectively, and the part it may play as a supplement to, as well as a substitute for, other channels.

One important characteristic of the vibratory signal, induced either electrically or mechanically, is that it cannot be "shut out". In this respect the skin is like the ear, unlike the eye. For many practical problems, basic data in this area that need to be obtained concern signal to noise ratios.

The tactile sense is normally itself a feedback channel, a part of a closed loop system, as it were. Lathe operators report that the changes in "feel" tells them when to make machine adjustments. In this area cutaneous vibration may be able to make some unique contributions once we code the skin to feedback signals.

With the assumption that enough of the basic problems of "what the skin can do" have been solved to convey adequate information for complex information processing, our problem becomes one of dealing with perceptions, judgments, and decision. Of course, an entire problem area exists in perceiving tactile impressions, distinguishing their properties and characteristics. "Paying attention" leads us into the whole realm of vigilance studies. Perceptions get categorized into classes, judgments involve comparisons of quantities along some dimension. It is here that psychophysical data become so important in our man-machine system. Decision rules and procedures become quite important to the system. Let us take the tracking problem of landing the pilot by vibratory signals. Here error must be minimized, a decision rule must be imposed as to how much variance is allowable. Since there is a limit to the amount of information that can be held in immediate memory at any given time it may well be that these unusual cues have an advantage when needed for immediate recall.

Let us now return to some of the problems of electro-pulse communication and describe some researches presently underway in the Psychology Laboratories at Carnegie Tech.

At present the important stimulus variables and parameters needing extensive research in order to describe the boundary conditions between "pain" and "pulses" include the effects of intensity, polarity, pulse duration, pulse interval, electrode size, spacing, electro-anatomy, and a better knowledge of local skin areas being stimulated.

The specific aims of our particular project are to identify the dimensions, parameters, and conditions for pain-free electric stimulation of the skin commensurate with the demands of an easily learnable coding system providing for a cutaneous language of practical use to the deaf-blind person. There are three phases to our present program of research. One phase is the psychophysical, which will be outlined in this paper. A second phase involves the problem of coding and complex information processing, which is being presented at this conference by my colleague, Dr. Lee W. Gregg. We have given consideration to the third phase of our problem; namely, teaching the new code, but we shall not discuss this problem at these meetings.

Two approaches have been discussed in planning our research program. In the first place we have considered concentrating our efforts in the beginning entirely on the psychophysical problems in order to find out the interrelationships between such stimulus dimensions as depth, duration, frequency, intensity, interval between stimuli, onset, and skin loci. Important to know about are such stimulus variables as acuity, adaptation, after effects, gamma and tau effects, and the possibilities of finding people with skin deafness. The problems here seem endless, so let us restrict our efforts.

We have decided to set up our program in a second way; namely, to attack selected psychophysical and certain coding problems at the same time. Desirable as it might be, for example, to know frequency-intensity-duration-resistance functions at many skin loci, we are asking the question of what information is needed for appropriate coding of a language for the deaf-blind, bearing in mind that they must communicate with people who may not be motivated to learn some difficult or radically new code. In other words, we are asking the question of what psychophysical information do we need, rather than attempting to learn all about skin sensitivity per se. Operating within such a frame of reference in building a cutaneous communication system, we believe it is important to emphasize from the beginning some of our practical needs. What do we need to know, for example, about the skin where relational comparisons are important, where there is a demand for rapid referability, or where some aid to vigilance is demanded? What unique problems are present where previous habits are not auditory or visual? Is it possible to come up with a language for the skin even more efficient than that mediated through the eye and the ear?

The apparatus we now have in use was constructed by our research assistant, Mr. Wayne A. Dempler, and based with some modifications on the design described in 1958 by Hahn (7). Square wave pulses are generated from Tektronix equipment and capacitance coupled to a triode amplifier utilizing a type 24 power triode to "stiffen" the pulses so that the capacitance and resistance of the skin does not distort the waveform. The output of this amplifier is applied to the electrodes. An oscilloscope is used to read the voltage at the electrodes. Hahn's basic circuit was modified to eliminate any direct current that accompanied the pulses. A resistance meter is inserted at the electrodes to keep a constant check of skin resistance. The pulse entering the skin can be described electrically. With the voltage readings obtained directly from the oscilloscope, power and current are calculated. Data are punched directly onto cards by using a keyboard wired to a relay station which in turn is wired to an IBM key punch. Each card contains identification code, and information as to the subject, stimulus conditions, voltage and resistance readings, used for computations which have been programmed for a "650" computer.

To date we have made a series of exploratory studies on some 50 normal human subjects and two blind subjects. We have evidence that

it is possible to get painless pulses which we believe will be suitable for coding. For example, some of our pilot studies determining lower thresholds using square wave stimulation, with duration held constant at 1 msec. and frequency at 1 p.p.s., show that voltage and current are rather stable over as long a period as one hour of stimulation. Skin resistance soon levels off after the first few stimulations and holds an almost constant value for the remainder of the stimulating session on any given skin area. Computed values for energy soon rise to an approximately constant value.

We have found it possible to repeatedly stimulate, slightly above minimal threshold, a given skin area at 1 p.p.s. with a duration of 1 msec. on the dorsal surface of the forearm up to two hours without reports of pain. However, at the end of such sessions the skin area has become very red in appearance. This picture of irritability may last several hours.

Holding frequency constant at 1 p.p.s. and varying five different durations in a geometric progression chosen arbitrarily between the limits of 0.05 msec. and 50.0 msec., we have found that in general as duration decreases, voltage necessary to attain threshold value increases. We have found that values around 50.0 msec. duration usually yield painful sensation even at threshold.

Our preliminary studies support the data of other investigators that frequencies in excess of 8-10 p.p.s. at most any duration will induce pain. Whereas we may be able to get pain-free pulse sensations on one skin locus another skin area yields pain under the same conditions of duration and frequency. Some skin areas yield only pain under any conditions of stimulation. Those skin areas where pain-free pulses can be sensed day after day can easily be made painful by increasing any one of the dimensions of intensity, duration, or frequency. We have found that we get our best results in obtaining pain-free pulses by using tantalum electrodes and by not using electrode paste. Size of the stimulating electrode seems to be of little importance.

We have our equipment so designed that we are able to present two sets of pulses simultaneously. Some exploratory data indicate that measurements of the two-point limen are related to whether the two active electrodes are in a proximal-distal or medial-lateral direction in placement. The sensation of "twoness" appears, under certain conditions, to be of the order of mere millimeters of separation as we go around the arm, a matter of inches up and down the arm. Certainly the spacing of electrodes might well be of importance in any attempts at stimulating the skin in more than one place. And it appears to us that in setting up a language system one of the really important considerations is one of the "several eyes" or "several ears" kind of consideration. The big advantage of the skin is that it has many of these "severals" if we can find conditions under which the different skin areas can be made separate channels for information input.

In Project Report NR 140-598 (1 Sept 1958) from the Virginia Laboratories it was stated that pulse durations ranging from 0.02 to 0.30 msec. and frequencies of 1 to approximately 8 p.p.s. under certain conditions elicit reports of "vibration" or "taps." Frequencies in the 10 to 15 p.p.s. range brought about reports which were often that of "pain", with the likelihood greater for durations of 0.2 msec. than for durations of 0.05 msec. At 20 p.p.s., the report was usually that of "pain", regardless of the shortness of the pulse duration. At 2 p.p.s., with durations as long as 0.3 msec., the report at threshold was "taps" or "pulses." These data were reported on stimulations of the volar forearm. In some of our data on the forearm we obtained somewhat similar results. However, we found the same stimulus package that would produce "pulses" on the forearm induced "pain" on the calf of the leg and instep of the foot. Skin locus is certainly an important variable in the problem.

We are now systematically making measurements which we hope will yield some data on the stimulus conditions that will produce pain, and the conditions that will give painless "pulses" which will have enough discriminable aspects for coding.

Mr. Melvyn Croner, one of our graduate students, has worked out a 5 x 5 latin-square design which he is now using in studying intensity-duration functions. He hopes to determine what durations (for any given skin locus) with their corresponding threshold values can be used without inducing pain, and how subjects vary from day to day in sensitivity. With the extension of this model we plan to study other dimension combinations. We are hopeful of coming out with a "package" of dimensions, yielding pain-free pulses useful in coding. Once we have the limits in which we can operate for a "pulse language" it is our plan to program the pulses for code learning. We have been told that this will not involve extensive technical difficulties, only money.

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Some Coding Problems in the Design of a Cutaneous Communications Channel

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The point of departure for the ideas I wish to present is the question "How can we utilize trains of electrical impulses to the skin as a means of communication?" The structure and properties of a cutaneous communication channel will depend, of course, on the purpose that the channel is to serve. Different criteria for evaluating the system are available. The particular criteria we choose stem from what it is we wish to accomplish.

Certain design problems disappear and others take their places as we envision different uses for the system. The construction of a simple warning device using the skin as an auxiliary channel scarcely raises the problem of an appropriate code--the problem to which most of the comments in this paper are directed. Instead our concern might well be the reliability of the device; or its comfort and convenience in use; or its cost in production. At the other extreme we could take on the task of producing a system which provides a completely new artificial language, the elements of which are defined by cutaneous sensations.

I will argue that we should work within the framework of an existing language--say English. Furthermore, in our conception of the need for a means of communicating via signals other than those appropriate to the visual or auditory modes, we should seek a structure that allows for all the richness and complexity that we now experience through our written and spoken language. In the development of this system, I will even argue that through the applications of electrical impulses to the skin we should be able to provide the rough equivalent of both auditory and visual stimulation.

The way I propose to elaborate the ideas on coding is first to consider some very general evidence concerning the rate of reception of auditory and visual signals. From this analysis, certain requirements for defining the processes by which human receivers interpret sensory data will be derived. Next, these requirements will be used to construct a simple theory that purports to explain how people interpret a particular code. This will finally lead us to some statements of the coding problems and to suggestions for research on the skin and other senses.

Reception of Auditory and Visual Signals

A variety of codes and conditions for receiving them exist as visual and auditory inputs for the human receiver. We will take as the ultimate purpose of presenting these signals the generation in the receiver of ideas--thought, relationships, and the like. We will assume that much of what passes as thinking is in fact the occurrence of strings of such ideas generated via language symbols, or more exactly the internal representations of words.

The word "chair" is a symbol that designates an object with which the receiver has had prior experience. The word may be externally generated by means of the written symbols of the English alphabet--a letter, code, or, through an alphabet of elementary sound signals--a phonemic code. Table 1 presents a number of ways of representing the flow of such inputs.

Table 1

Rate of Receiving and Coding Flow

<u>Activity</u>	<u>Primary Code</u>	<u>Intermediate</u>	<u>Reception Rate</u>
Spelling aloud	Speech Sounds (39)	Letter Code (26)	Words 50/min.
Spoken production	Speech Sounds (39)		Words 250/min.
Reading, silent	Visual Patterns (?)		Words 500/min.
Receiving Morse	Auditory Signals (5)	Letter Code (26)	Words 75/min.
Reading Braille	Tactual Patterns (40)	Letter Code (26)	Words 60/min.
Reading Shorthand	Visual Patterns (?)	Speech Sounds (39)	Words 200/min.

There are two ways to characterize the differences among the activities listed in Table 1. The first implies that constraints are imposed on the receiver by the code or circumstances of receiving it. In listening to another person spelling words aloud, the receiver must first interpret the speech sounds as letters of the English alphabet and from these obtain the words. The 5:1 rate difference arises from the fact that the measure of a "word" is five letters of the written alphabet. If we assume that the speech sounds that give rise to the letters are produced no faster than the speech sounds that give rise to words themselves, dividing the maximum sustained rate for understanding the spoken words by five gives the rate for spelling aloud.

A comparison of spoken production with silent reading suggests a constraint determined by the method of presenting the code. The rate of reception depends on the rate of the speaker's production in the auditory case. The listener must wait for the next sounds to come forth, but dare not wait too long for a sequence of code elements to accumulate lest he forget the initial elements. A page of printed text gives the reader an opportunity to scan ahead, or if he forgets, to refer back to prior symbols. He can set his own pace. His comprehension, the reception of ideas, is greatly facilitated.

There is another quite different approach to the characterization of the differences suggested by the rows of Table 1. Does the two-stage decoding suggested for spelling aloud, receiving Morse Code, and reading Braille truly exist in the interpretive processes of the human receiver? Is it not possible that the fairly consistent 50, 60, and 75 word per minute rates demonstrate an artifact based on the amount of experience the receiver has with the particular code? Does the blind reader or the expert telegrapher in fact deal with an individual letter code?

Stage of practice may determine to what extent a receiver can improve reception by internally recoding whatever primary alphabet is used in sending the message. The child in first learning to read may follow an interpretive processing procedure similar to that claimed by Flesch.

Visual Patterns —> Letter Code —> Speech Sounds —> Words
This sequence implies that individual letters of a word must be attended to and recognized, then speech sounds are generated by the combinations of letters. Only from these sounds, the sounds of the spoken word, is it possible for the child to recognize the word. Later the visual patterns lead directly to words. With sufficient practice in Morse, Braille, or

spelling aloud, the upper limits may approach other forms of communication activity.

One inference we might draw from the above is that the properties of the primary code are quite unimportant since the receiver will suitably modify his internal representation of whatever externally presented primary alphabet is used. However, most of us would reject the notion that transmission rate is independent of the properties of the code, and we would do so on other grounds than that the time required to learn the new code could be excessive.

The argument would run something like this. First, assume that the channel capacity of the sensory system is fixed. Just as Fitts (1954) tried to show that the motor system exhibits a reasonably constant capacity, we might suppose that a particular sensory channel must have an upper limit. What gives rise to the apparent discrepancies in the rates of Table 1 is the fact that not all of the information generated by the source is processed. Spelling aloud requires the interpreter to act upon each individual letter symbol. Interpretation of connected speech does not.

Internal re-coding processes can be thought of as techniques that enable the receiver to take into account the redundancy present in the message and extract the "basic" information content. Optimum codes are those that provide the most efficient match between the structure of the language in which the messages are given and the information processing mechanisms of the receiver. Figure 1 depicts the relationships between language structure and code; between code and interpretative processing.

To make the latter relationship explicit and provide a basis for testing some specific ideas concerning the nature of the cognitive organization capabilities of a human receiver, I have attempted to state some requirements for an interpretive system and from these present a theoretical picture of the way such a system must behave in the specific context of interpreting Morse Code.

Requirements for Code Interpreter

1. Discrete segments of the input signal are intermittently sampled by the system. This requirement is independent of the continuity or discontinuity of input itself. The requirement implies that size of the segment and rate of intermittency are important parameters

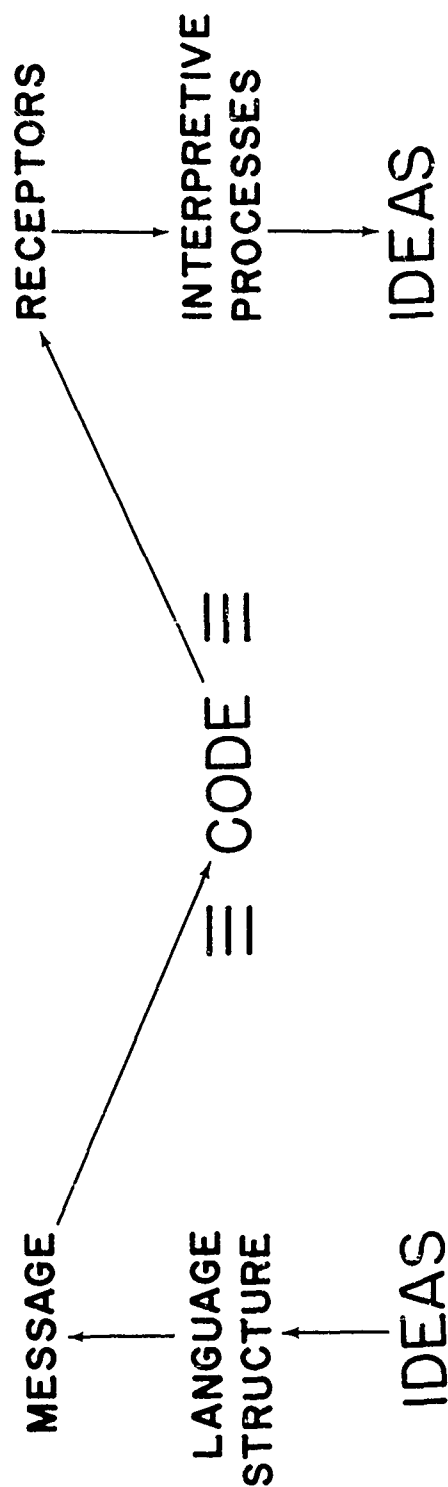


Fig. 1. An optimum code must match the language structure and the interpretive processing acts of the receiver.

for system performance. The definition of the segment can be a function of either spatial or temporal aspects of the signals.

2. The system must provide a detector mechanism that serves to differentiate among the coded signal elements.

3. An assumption and a requirement for a system intended to simulate a human receiver is that there is limited immediate memory storage capacity for input segments not processed as above. This requirement limits the size of the segments suggested in the first requirement and implies an interaction with the amount of processing called for by the second requirement, because . . .

4. Requirement 4 is that the system operates as a serial machine. Only one active process is in control at any time.

5. A permanent memory structure contains the associations between signal elements and things for which they stand. One or more "information retrieval processes" make it possible to go from sign to designatum.

Computer Simulation of a Code Interpreter

Let us now attempt to formulate these requirements as a precise set of statements called a "program." This program is in fact a theory of the information processing behaviors of the receiver whose task it is to decode or interpret sensory inputs. The program is written in such a way that a digital computer will exhibit behaviors evoked by first the program and second by an "environment" producing the signals. Moreover, it is hoped that the computer's behavior will simulate that of a human receiver placed in the same "environment."

Computer simulation of complex human information processing is a new method by which cognitive acts may be described. For this reason we should perhaps look a little more closely at what it is we attempt to do.

This methodology grew out of the work of Newell, Shaw, and Simon (1958). The nature of the programs can best be described as "heuristic." By this we mean that the high speed capabilities of the computer to perform certain simple repetitive tasks are played down in favor of the use of selective, organizing properties of the data stored in and presented to the machine. This is not to say that iterative features are completely absent. Most of the programs have many recursions to and

through loops previously employed although usually with changes in the inputs to these processes.

There is now available a "public" computer language called IPL-V, where IPL stands for Information Processing Language. Using this language, a computer (IBM 650, 704, 709) is able to perform a variety of primitive processes on symbolic units. These primitives may be combined in an indefinitely large number of ways to produce other processes, the characteristics of which are determined by our ideas concerning the system we wish to simulate.

We do not suppose, in general, that the time required for the machines to perform these processes corresponds in any absolute way to the time consumed by the human doing the same thing. We can, however, define what we consider to be the primitive processes of perception, cognition and the like, make estimates for the duration of them, and by summing over the primitives used in the more complex processes arrive at a suitable time scale. This concept will be important for our subsequent considerations.

Morse Code Interpreter

The specific code that I have used as a basis for constructing the theory is the International Morse Code. The level with which we are dealing with the code is quite different from building a machine that automatically decodes the dots and dashes and produces a printed copy of the message. That problem, incidentally, is not a trivial one, at least for the case where the sender is a human operator (Gold, 1959; Blair, 1959).

Bernard Gold (1959) recently described a special purpose digital computer that attempted to do this. What made the task formidable was the variability of the signal durations. It is interesting to note that in the final solution--a machine that decoded connected text which could "easily be read by a man who knows the language"--the properties of the code and the properties of the language being sent played an important role in the structuring of the decision rules for determining dot, dash, short space, and long space. Blair (1959) employed a general-purpose digital computer. Both machines depended on very large immediate memory storage so that statistical distributions of the durations of code elements could be obtained, thus making possible the assignment of the "true" values to their approximations.

We will assume that the human receiver can perform the necessary sensory tests with complete accuracy. Thus, we eliminate for the moment those aspects of interpretation that were of primary concern in the Gold and Blair machines. The IPL simulation proceeds as follows. The coded input is represented as a stream of input symbols R0, R1, R2, and R3, where these symbols stand for the dot, dash, letter space and word space, respectively. Associated with each of these events is an attribute, the value of which leads directly to the internal designation of that symbol. The way that attributes are "associated" with objects is through the use of description lists. In structure, then, we have a symbol that is the external event, attributes of that event, and a process that finds the value of the attribute. Finding the value of the attribute which we shall call A0 is the equivalent in simulation to recognizing the external event as a dot, dash, or space. We might have:

R0	90	0000	List for "dot"
90	00		Description list for R0
	A0		Attribute
	V0	0000	Value

The name of the process that finds the value of an attribute is J10 and we would have to name the object Rxxxx and the attribute Axxxx for J10 to do its work. Thus a portion of our program might read:

R0	name of event
A0	name of attribute
J10	find the value of A0

The symbol V0 would now be available as an object for subsequent processing. In IPL-V a special cell on the computer, called H0, serves as a "communication cell." Into it are placed the inputs for processes such as J10 and upon completion of the process we find the result in that cell. It is convenient, however, to keep the cells which have psychological significance distinct from those that are employed in the basic operation of the IPL computer. The next step, therefore, is to name certain cells which will serve as the immediate memory cells of the interpreter.

The first of these cells we shall call M0, the image cell. In it we find the name of a list on which is the string of external symbols--the

R's. The maximum length of the list is determined by the immediate memory span of the receiver. This list, then, is the intermittent sample of the input. It makes available for subsequent processing a certain number of external code symbols. Notice that in describing Morse Code with its variable durations for the separate code elements we have introduced a simplifying condition; namely that the elements are of the same duration. We would expect our predictions to be somewhat inexact because of the confounding that actually exists between symbol duration and number of symbols. We will use the "magic number" seven (Miller, 1956) as our initial setting for the length of list and the regional symbol Sxxxx to name segments of the input.

The distress call SOS, SOS ... — — — ... — — — — —, would take the form:

S0	90	S1	90	S2	90	S3	90
	R0		R1		R0		R0
	R0		R1		R0		R0
	R0		R1		R0		R0
	R2		R2		R3		R2
	R1		R0		R0		R1
	R1		R0		R0		R1
	R1 0000		R0 0000		R0 0000		R1 0000

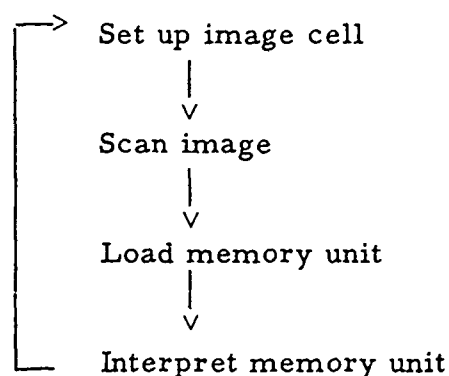
A rule is implied by the form of the segments that a new segment must begin at a point in the series that follows a word or letter space. The psychological import of this rule is related to the way a novice receiver would tend to look for letter units, spelling out the message one letter at a time.

But we have not as yet represented the dots and dashes of the code as internal symbols. To do so we must define additional memory cells, M1 through M6, which will contain the values of the A0 attribute for each external symbol. These cells are placed in order on a list M10 called the memory unit. A portion of the program called the image scanner takes successive symbols from the current image segment, finds the

values of the attributes and places that value in the memory cell that corresponds to the ordinal position of the external element on the image list. At each step a test is performed to determine if the symbol is a mark or a space. If a space occurs, the content of the image cell M0 is changed to the next segment and the program branches to make use of the values in the memory cells of the memory unit as the basis for finding the letter specified by those values.

Our conception of the permanent memory structure of the interpreter is that of a branching network consisting of two kinds of cells--test cells and terminal cells. The terminal cells can be thought of as the locations of the letters of the English alphabet. They are indicated by the square blocks of figure 2. The test cells are of two kinds. The first asks the question of the contents of a cell in the memory unit "Am I a mark or a space?" If so, the + branch leads directly to a terminal cell and the name of a letter. The second test distinguishes between the dots and dashes (both are marks). Since the interpretation of the memory unit is completed whenever any space occurs in the next cell on the M10 list, it is unnecessary to distinguish between the word and letter spaces.

A selection process, then, is triggered by the occurrence of a word or letter space as the interpreter scans the memory image. This process makes use of a fixed list, M10, on which we find the names of the memory cells M1 through M6. Upon finding a terminal cell, the retrieval process returns control to the image scanning process.¹ The operation cycle is:



As presented, this system may exhibit certain features in common with a human subject who has just memorized the dot-dash equivalents

¹An operational program that contains the essential features of the code interpreter and further is capable of performing simple rote associations was devised by Feigenbaum (1959).

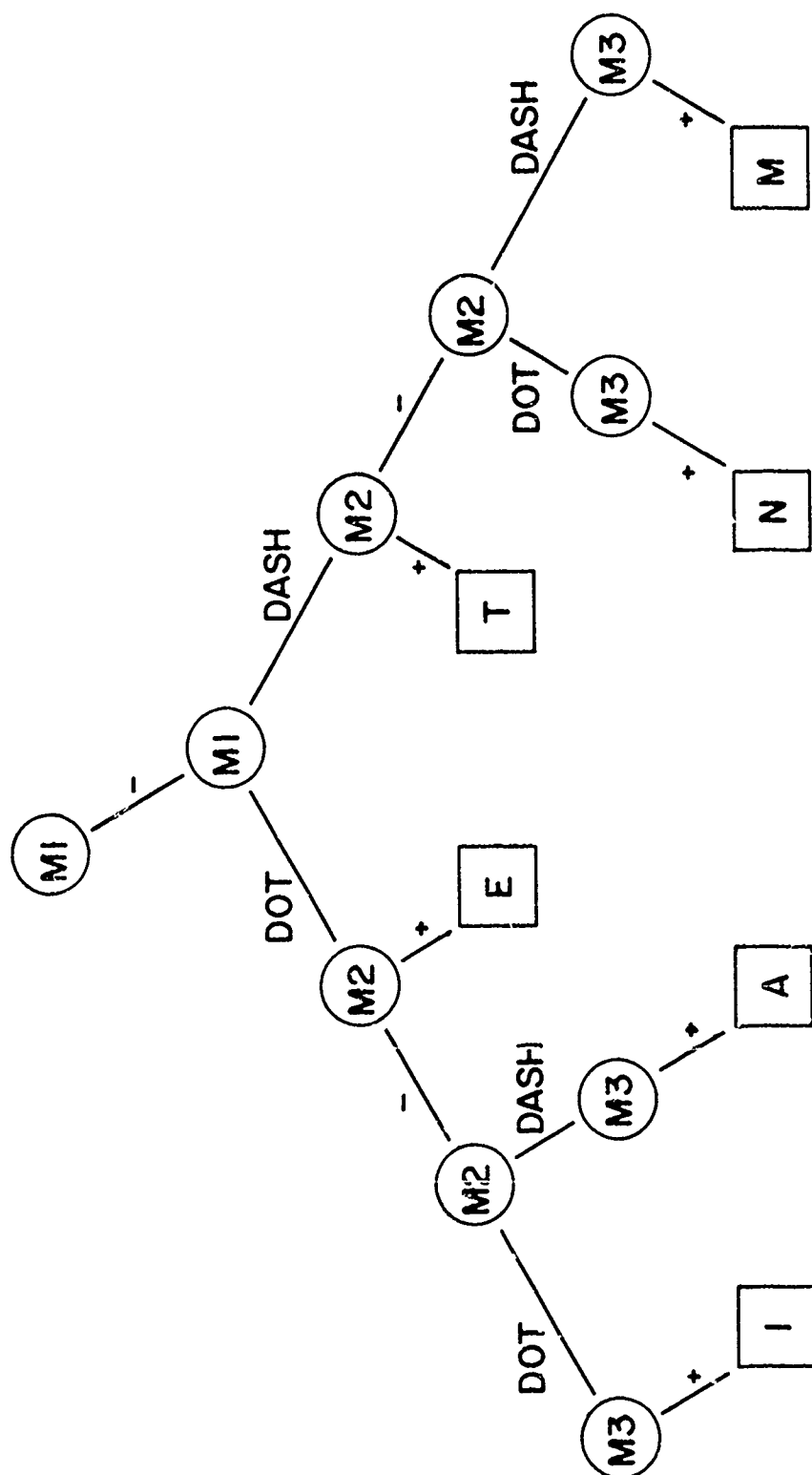


Fig. 2. A portion of a discrimination net for letters of the alphabet. There are two binary tests for each cell in the memory unit. These lead to the definition of the code symbols. The first test is 'mark or space' where the minus sign means mark and the plus sign means space. The second test distinguishes between short marks (dot) or long marks (dash). Interpretation of the contents of the memory unit is completed when a space symbol is found. The symbols M1, M2, M3 indicate that the process which interprets the net obtains the information necessary for its operation from those cells in the memory unit.

of the English alphabet. This subject must--the theory states--pay particular attention to the spaces in interpreting the code and will attempt to produce the letter equivalent for brief strings of Morse signals no more than five or six in length. He will do so by a process that says essentially "the first one is a dot, the second one is a dot, the third one is a dot, the fourth one is a dash, the fifth one is a space. . . dot, dot, dot, dash. . . oh yes, that's the letter V."

There is ample evidence (Taylor, 1943a, 1943b) that well-practiced receivers do not behave in the way we have described. In fact, Keller (1943) recommended that the dot-dash terminology be omitted in training and that the letter signals be presented at a rate of 15 words per minute although with intervals between the letters of 3 seconds. The skilled receiver exhibits the phenomenon of lagging behind the message by whole words or phrases. It is very improbable that he is able to do so because practice significantly improves or extends his immediate memory span. Rather we would suppose that he has learned selectively to process portions of the incoming message. And to do so, he has very likely modified his internal representation of the external signals.

At present we can do little more than suggest some of the possibilities whereby Ss may internally recode the external signal elements. It is certainly of no value to say that the receiver simply perceives the larger patterns as a Gestalt. There must be distinguishing features of the larger units that S depends on for recognition. We can perhaps steal a trick from the Gestalt hand and propose that a figure-ground distinction may be useful in pattern recognition. Consider how this concept gives rise to a new form of sensory test. The receiver hears a string of elements most of which are dots. These are periodically interrupted by long spaces. The dots become the ground against which the figure--the location of the dash in the sequence--stands out as an identifying cue. We have evidence from two quite different experiments that many Ss do just exactly this kind of structuring.

In one experiment, S faces a panel on which is mounted a set of four switches. The switches may be moved to the left or to the right. A total of 16 different configurations of the switches is therefore possible. The S must learn a particular sequence of these configurations and carry out search for each successive correct configuration. Some of the sequences we have used were random. Thus S was essentially learning a list of four element consonant words--a list of high intralist similarity. Some of the sequences were constructed by rules of a fairly simple nature (to the experimenter) such as counting by the binary numbers. In many of the experiments Ss were asked to describe aloud what they were doing

as they searched for the correct responses. In others, the Ss simply performed until they solved the problem or learned the sequence, after which they were questioned about their learning. One of the most frequent ways of describing the switch settings was that of naming the switch or switches that departed from the "major direction" or standard position of the switches, e. g., all to the right or all to the left.

Similar inferences about a figure-ground test were derived from another experiment just completed. This time the inference is tied down with somewhat more objective data. The task was patterned after the typical memory span experiment. Digit words of length 5, 6, 7, 8, and 9 symbols were tape recorded at a rate of one symbol per second. For half of the Ss the lists were composed of the symbols "one" or "two" while for the other half, all of the digits from one through nine were used. A second independent variable was defined as "random" versus "systematic" presentation of the words. For the systematic condition all the words of the same length were presented together. Ss reproduced one at a time a total of 100 digit words, 20 of each length. As before Ss reported upon their method of retaining the information, and as before a number of the 40 subjects in the groups where only two values of the symbols were involved reported that they attempted to remember the locations of the symbols that were different from a field defined by symbols of the other value. The very superior performance of the two value, systematic Ss leads us to suspect that the sort of internal recoding that we are suggesting here plays an important role in such serial tasks.

Let me suggest a second possibility for internal recoding. We know that human Ss are very good in estimating brief temporal durations and in successive comparisons of such intervals (Gregg, 1951). A method of recoding that provides categories of long or short symbol strings could be applied to larger units of the memory image. This test coupled with a "numerosity" test yielding the number of code elements within the symbol string rather than their dot-dash values could provide ready access to a very large number of letter distinctions.

The implication that internal recoding leads somehow to faster recognition of the larger code units is obviously based on a difference between the application of the sensory tests as discriminatory mechanisms and the search through the various branches of the permanent memory store that we have called the discrimination net. We are stating as a problem, open presumably to subsequent test, that the processes of recognizing elements, whatever their size may be, of a

fixed alphabet of symbols will proceed very much more rapidly than the processes of interpretation. The latter utilize the fixed elements to provide sufficient variability and complexity to convey the information content of the language. Our simple theory states why this may be so by providing for the elementary code symbols a set of fixed sensory tests in a fixed order of application. Thus, when we say that a process finds the value of the symbol R0 directly we mean that the discriminations as depicted in the net of figure 3 can be performed in a shorter elapsed time than the same number of binary discriminations in the net of figure 2.

Stated as an empirical proposition this distinction between sensory test and cognitive search of the permanent memory would predict that the time required to recognize a complex object would be shorter when the order in which elemental informational attributes are presented is fixed than when that order varies.

But to assume that all of the increased efficiency in handling the serial input accrues from the recoding processes alone would be a mistake. The fundamental reason for the internal recoding schemes (we suppose) is to make it possible for the receiver to be selective in his search for the meaning of the coded message. Without recourse to elaborate linguistic analyses, a task that should be carried out if we are to make the code match at both ends, we can nonetheless propose a few simple devices whereby the receiver through context of the message itself or through inputs from another separate channel, profits from the opportunities set by the internal recoding.

As a first illustration based on context, let us suppose that our interpreter has learned the Morse equivalents to the numbers as well as the 26 letters and in doing so has noticed that all of the numbers are represented by five code elements. As part of the memory unit we provide a new cell, say M7, which has in it a symbol that defines the context of the message. The symbol, of course, must be updated by periodic analysis of the results of prior inputs. The symbol in the cell M7 serves as a pointer to various test nodes of the main storage; it gives priority to the performance of certain ones of the various possible tests that may provide access to the number cells of the memory. If the context changes, errors or decreases in speed of interpretation may result. As with all heuristic devices, there is no guarantee the mechanism will always work properly. The important point is that such a mechanism could only work for a coding procedure other than the serial dot-dash-space representation.

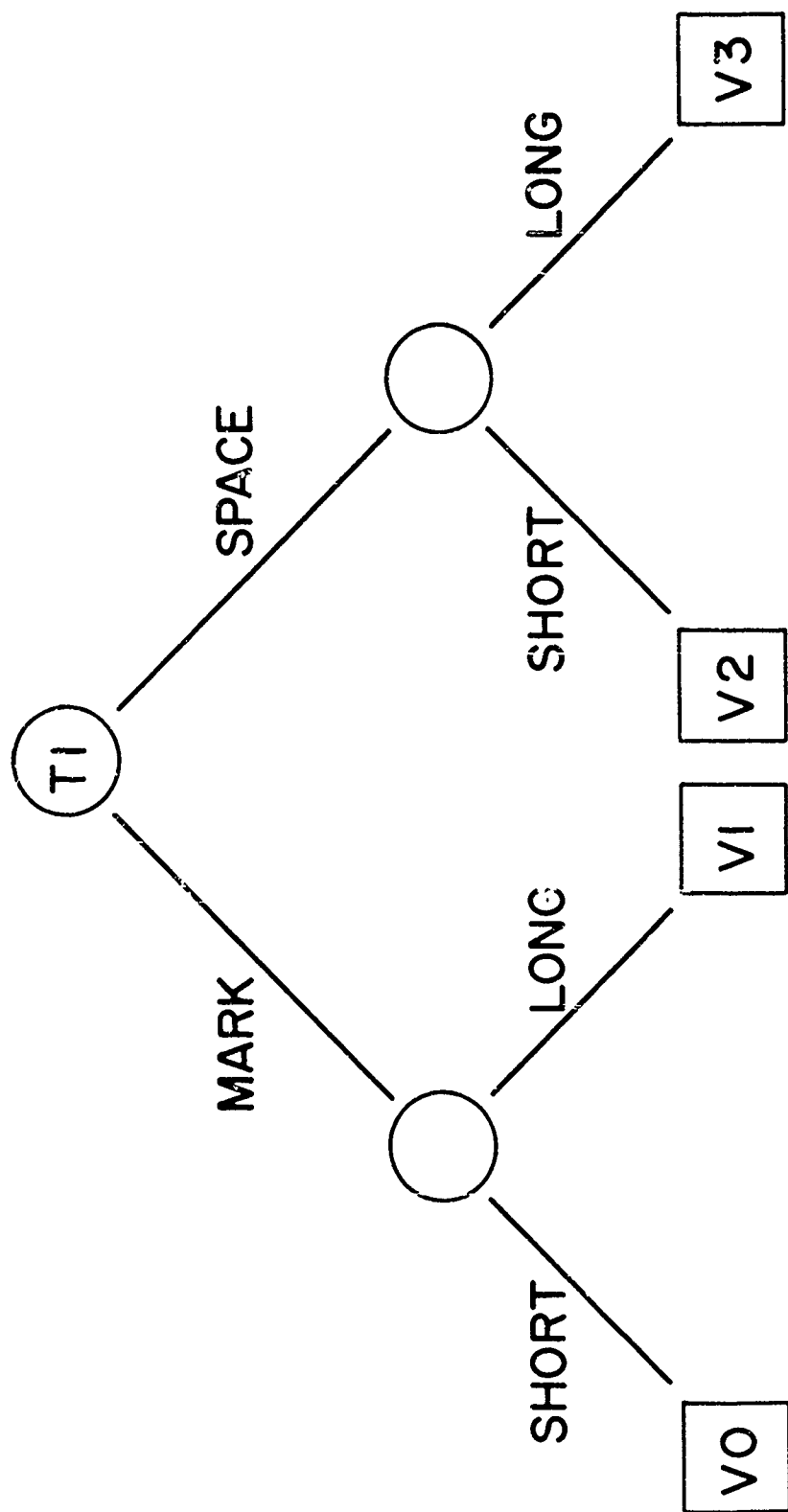


Fig. 3. Discrimination net for code symbols. The symbol VO is the internal representation of the external code symbol RO, dot. The symbol V1 is the dash; V2, the letter space; and V3, the word space.

Context set by inputs other than the primary message source are obvious at levels of analysis beyond that which we are concerned with here, but there is no reason to deny the possibilities of interactions at the lower level. The visual stimulation of a person entering a room sets one for messages of greeting. Cognitive expectations of subsequent stimulation are induced by sights, sounds, smells, and a wide variety of associations. These ideas are as old as language itself but by calling attention to them and providing a technique that forces a precise statement of them and a means for organizing them into a functional system we may find ourselves in a better predictive position than heretofore.

Problems of Coding and Sensory Research

We now attempt to define the fundamental problem of coding and to show how our speculations concerning the nature of the cognitive processes utilized in code interpretation help determine the properties of an optimal code for the English language. First, it is necessary to recognize that the problem as initially stated requires that an indefinitely large number of words must be created from an alphabet that has a finite set of symbols. The case where the communications system is used to convey but a limited number of possible words--here the equivalent of message ideas--with each word represented by a single symbol of the code is considered trivial. This coding algorithm is called dictionary coding and it is obvious that we would soon exhaust the supply of distinguishable sensations if we were to attempt it. This does not mean, however, that the only mapping possible for the words of the language is that of utilizing the letters of the English alphabet. We have, of course, the speech sounds and the imperfectly understood visual patterns that provide the cues for word recognition. Nor should we discard the possibility of a letter code based on some form of abbreviated spelling as in certain notehand systems.

In any event, every mapping of a set of words of a particular alphabet into a set of words of another alphabet is called coding. Our written alphabet as a means of representing words is well known to us and for this reason should perhaps be the basis for any cutaneous code. But we have the feeling that spelling words out letter by letter of the written alphabet is too slow. And since we know so little about the processes of word recognition it is doubtful that we could progress very far using the cues of the visual alphabet as a base. Our best bet, therefore, is to work from the phonemes of spoken production.

The system we propose to construct is one in which cutaneous sensations are placed in one to one correspondence with the elemental sounds

of speech. What can we learn from the code interpreter that will let us proceed intelligently with this task? Assuming that we are able to discover through it some of the basic information processes employed in perception and cognition, we should then be able to match our cutaneous code to that providing the auditory input of speech. Although tentative and inadequate for the interpretation of speech sounds the present code interpreter will serve to indicate the direction that we shall take.

1. What is the nature of cutaneous imagery? Given that a number of discrete stimuli can be produced without pain, what is the size of the immediate memory span for varying numbers of them presented at varying rates? Our concern here is with the contents of the image cell.

2. What combinations of levels and dimensions of the cutaneous stimuli provide unique sense "sample points" over variations in rate and intensity? We suppose that absolute intensity differences could serve at best as context cues and that fairly wide intensity variations should be expected in the operating system. Also, changes in transmitting rates should be expected. The problem is to define stimulus parameters that provide informational content that remains invariant over these changes. We are essentially asking about the nature of the sensory tests that will work efficiently.

3. How many distinct symbols based on the cutaneous stimuli can we derive and what is the duration of each? From this information we will be in a position for the first time to estimate the expected transmission rate for the cutaneous channel.

4. What are the possibilities for simultaneous stimulation over dual cutaneous channels? Without arguing the question of true simultaneity (Broadbent, 1958, pp. 34-35), we wish to determine the extent to which a separate channel could provide context-setting information. It is assumed that considerable utilization of spatial cues will be required for the code elements. However, gross separation as left arm versus right arm might be used to produce the dual channels.

Concomitant with the investigation of the cutaneous phenomena, we will seek a better understanding of the utilization of speech sounds in communication. Analyses based on information theory concepts seem to suggest that very much more information is provided the organism than it could ever possibly use. We feel that information processing concepts provide a much better basis for predicting the amount of assimilated or utilized information in the human receiver.

We are still lacking the appropriate analyses of the language structure that are undoubtedly related to the development of the cognitive processes used to interpret such structures. And obviously we are not prepared to deal with the facts of neural coding. We hope to start our education in the latter area at the conference.

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On the Potential of Tactual Displays: An Interpretation of Recent Findings

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For several years those of us directly involved in the cutaneous research at the Virginia Laboratory have been emphasizing the feasibility of tactual communication. I think it fair to say that from the standpoint of tactual sensitivity our point has now been established conclusively. The critical study involved a test system in which letters and numbers were encoded into tactually identifiable vibratory signals. Three durations, three intensities, and five locations of vibration on the ventral thorax provided the potential capacity for a 45-element system. Using this arrangement, we were able to train a man to receive words at a rate of 38 words per minute despite the fact that the coding scheme may have been something short of optimum (6, 10).

The practicality of tactual communication, however, is not solely a matter of feasibility from a sensory standpoint: there are other factors to be considered. First of all, a need for such a mode must exist. Second, a practicable method of presenting signals must be available. Third, a superiority over other modes, in one respect or another, should be demonstrable. Finally, any limitations of such a mode should be clearly recognized. It is to an evaluation of tactual communication with respect to these requirements that the rest of this paper is devoted.

There is little question that the need exists in a great many areas for some sort of addition to the existing communication facilities. The question of greatest relevance, then, is: In which areas would a tactual display serve to the best advantage? Let us approach this question by considering the types of information which it might become necessary to convey. One possibility is that of high-frequency, discrete signals characterized by a relatively high degree of uncertainty (uncertainty here referring only to the number of alternative events possible). Letters of the alphabet might constitute such a population. Another possibility is that of high-frequency, discrete signals with a low degree of uncertainty, such as the dot-dash of the telegraph code. A third possibility is that of continuous input signals such as characterize many control tasks. A final possibility is that of extremely low-frequency, discrete signals as

are likely to occur in monitoring tasks. We will consider the suitability of the tactual mode for displaying each of these types of information in view of our criteria and the existing empirical data.

For the high-frequency, high-information signal population, the tactual mode would be pressed to its limit. The implementation of such a system would, at best, be difficult. Furthermore, unless something drastic were wrong with the visual and auditory channels, there would be little point in investing months of training to bring tactual performance up to a level comparable with those channels, if, in fact, such a level could be attained. Indeed, there is little question that the tactual system is considerably less sensitive than the auditory or visual systems for resolving small intensive and qualitative differences. Neither, for that matter, is the skin frequently called upon to make such fine judgments, or to serve as a communication channel in any but the broadest sense. Both of these facts argue against the possibility that high-frequency, high-uncertainty communication would come easily to the tactual system. The fact that we have achieved a degree of success in such a display situation points up the potential of the tactual modality, but does not recommend its substitution for the eyes or ears except as a last resort.

If the amount of information in rapidly presented signals is reduced, let us say, to one bit (a two-choice situation), the practicality of using the tactual mode increases. For one thing, the reaction time for touch--particularly if stimulated electrically--is lower than that for vision and probably also that for audition. Thus, a potential superiority over the other channels exists with respect to rate of information processing, providing the two stimuli used are easily discriminable. Such ready discriminability is provided by the broad expanse of body surface: all that is necessary is a pair of stimulators well separated on the body surface to carry the information. In addition to the fact that such an arrangement is simple to instrument (always an important consideration in tactual communication), it has the advantage that all sorts of variations in intensity, duration, frequency, and even position of the stimulus can be tolerated. I use locus as the relevant cue in this example because I think it is the most promising of the possible dimensions for use in situations of the sort presently under consideration. The results of our work with multidimensional stimuli indicated that the five loci used were never confused (10), although they covered only a small segment of body surface. Subjects reported, in fact, that the "local signs" of the signals were inescapable. I suspect--and this is only a guess--that disjunctive reaction times would be lower for the dimension of locus than for any other.

Before I leave this discussion of locus, I would like to put forth one more matter for consideration. There are portions of the body which seem to be "natural" for encoding information in certain tasks. Individual stimulation of the fingers, for example, would seem an excellent way to display information in a key-pressing task. But aside from these few obvious spatial correlations, there is little to guide us in choosing optimally compatible stimulus-response sets. Since we do not typically use the tactual modality for communicating, there is little opportunity for us to develop learned relations or cultural stereotypes. Furthermore, it appears that there are occasions in which we must reject the intuitively appealing notion of physical correspondence, i. e., that the correspondence of stimulus and response sets in a direct physical sense guarantees compatibility. The question of how to maximize stimulus-response compatibility with locus as the stimulus variable, then, is one which demands an empirical answer. A first approach to this problem might be through the use of the disjunctive reaction-time paradigm. Using various response sets, we might ask: Are reaction times lower when we stimulate five fingers (as suggested above), five toes, or one finger, one toe, a leg, an arm, and the chest? I think that such an approach has many theoretical as well as practical implications. Briefly, then, it appears that for a wide variety of tasks, locus stands as the most feasible dimension for coding information, although its full potential cannot be realized until we know more about the relationships of tactual space and response topography. Even without this knowledge, however, we should have little difficulty in developing a tactual system for conveying high-frequency, low-uncertainty information efficiently: both from the standpoint of training demands and of asymptotic performance levels.

It should be noted, parenthetically, that in some situations (although undoubtedly not in most) the instrumentation of a single stimulator operating at widely separated values of intensity, frequency, or duration would be more practicable than multiple stimulators. The relative efficacy of such dimensions and of specific values of each to elicit various specific response classes constitutes another interesting problem for the reaction-time paradigm.

Intimately related to the high-frequency, discrete-input situation just considered is that in which a more-or-less continuous response is required to a continuous stimulus input: i. e., as in the typical tracking task. If a compensatory display is employed with three vibrators to indicate off-target to the left, on-target, and off-target to the right, the two situations are quite comparable. The major points of difference

involve the underlying continuous function and the confounding of input and feedback information characteristic of compensatory displays in general.

Several exploratory investigations have been carried out using tactual displays of this sort. In one of these, a great deal of success was reported using only two vibrators: off-left and off-right (3). In another, the tactual mode was found to be inferior to the visual mode for tracking in two dimensions.¹

More recently, my associates and I (11) undertook a series of exploratory experiments with a display composed of three vibrators spaced 7 inches apart on the chest. Performance on this display was evaluated through comparison with that obtained using two visual displays: one an analog of the tactual display with off-left, on-target, and off-right lights replacing vibrators, and the other a conventional cathode ray tube display with continuous error information. The comparisons were made at three input frequencies for each of three simulated systems: a positional control system, a quickened acceleration system, and a super-quickened acceleration system. In the quickened and super-quickened systems we expected that performance difference between discrete and continuous displays would be minimized.

The findings are summarized in Figures 1 through 3, which correspond, respectively, to the positional, the quickened acceleration, and the super-quickened acceleration systems. In all three we see that the vibratory mode is comparable to the visual discrete mode up to about 5-cpm input frequency. As frequency is increased further, however, the position of the vibratory display relative to the visual ones becomes increasingly poorer. This is true for all three systems. In fact, the only important difference between the three sets of curves (representing the three systems) is the expected one: a reduction in the discrepancy between discrete and continuous displays as quickening is introduced. Super-quickening, it seems, adds little.

Does this mean, then, that a vibrotactile display is only feasible at ultra-low frequencies in the continuous case? Not necessarily. There are several after-the-fact explanations which may account for the poor showing of the tactual mode at high frequencies in this study. One concerns the matter of learning. In the study involving the positional system, we extended training on the 5-cpm input over a period of some 80 25-sec. tracking trials and found that improvement was four times as great for the tactual display as for the discrete visual display. Learning on the

¹Research by P. Chinetti and D. Hunt, personal communication.

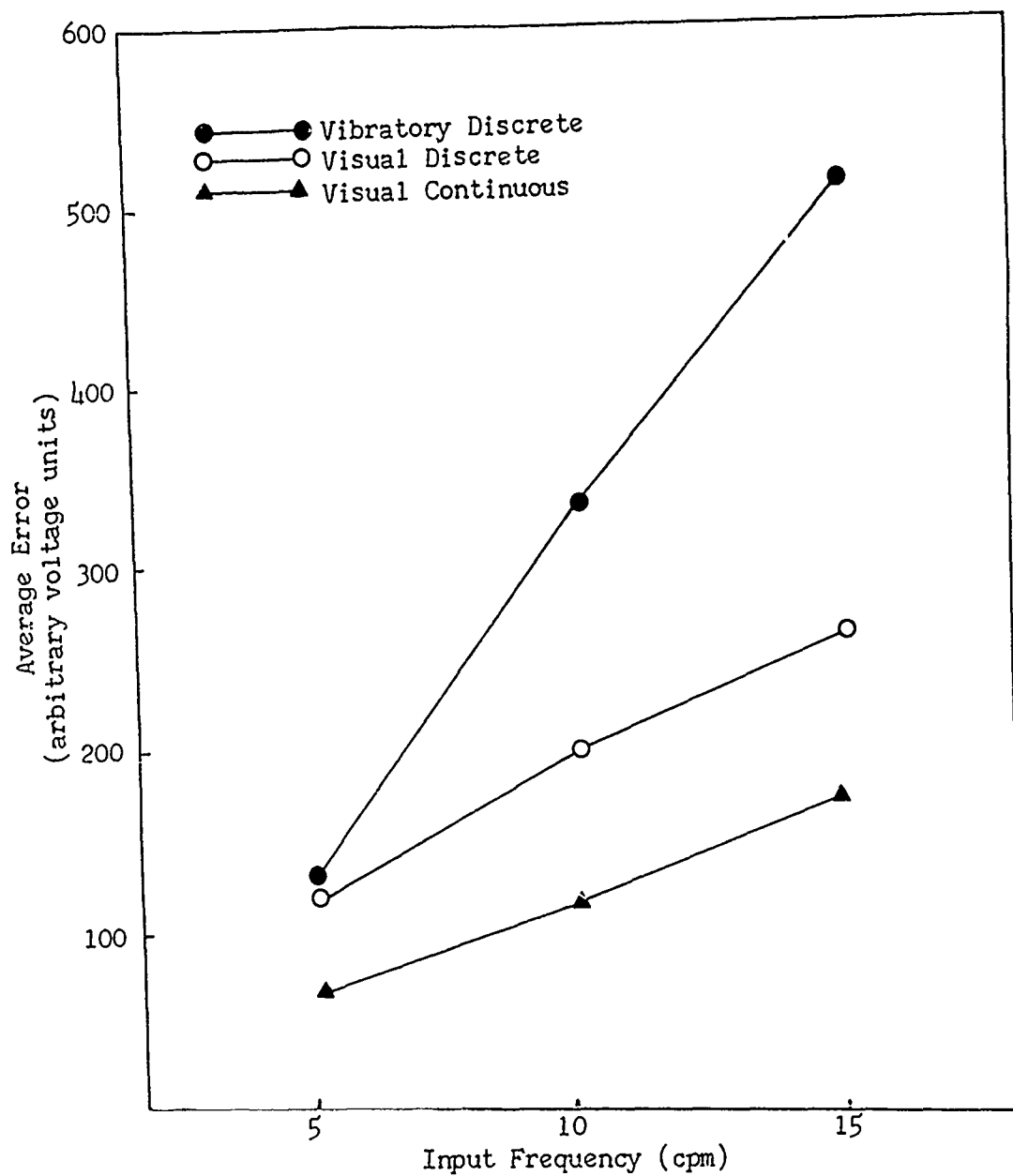


Fig. 1. Tracking error as a function of input frequency for three display modes in a positional control system. (Data from Howell and Briggs, 10)

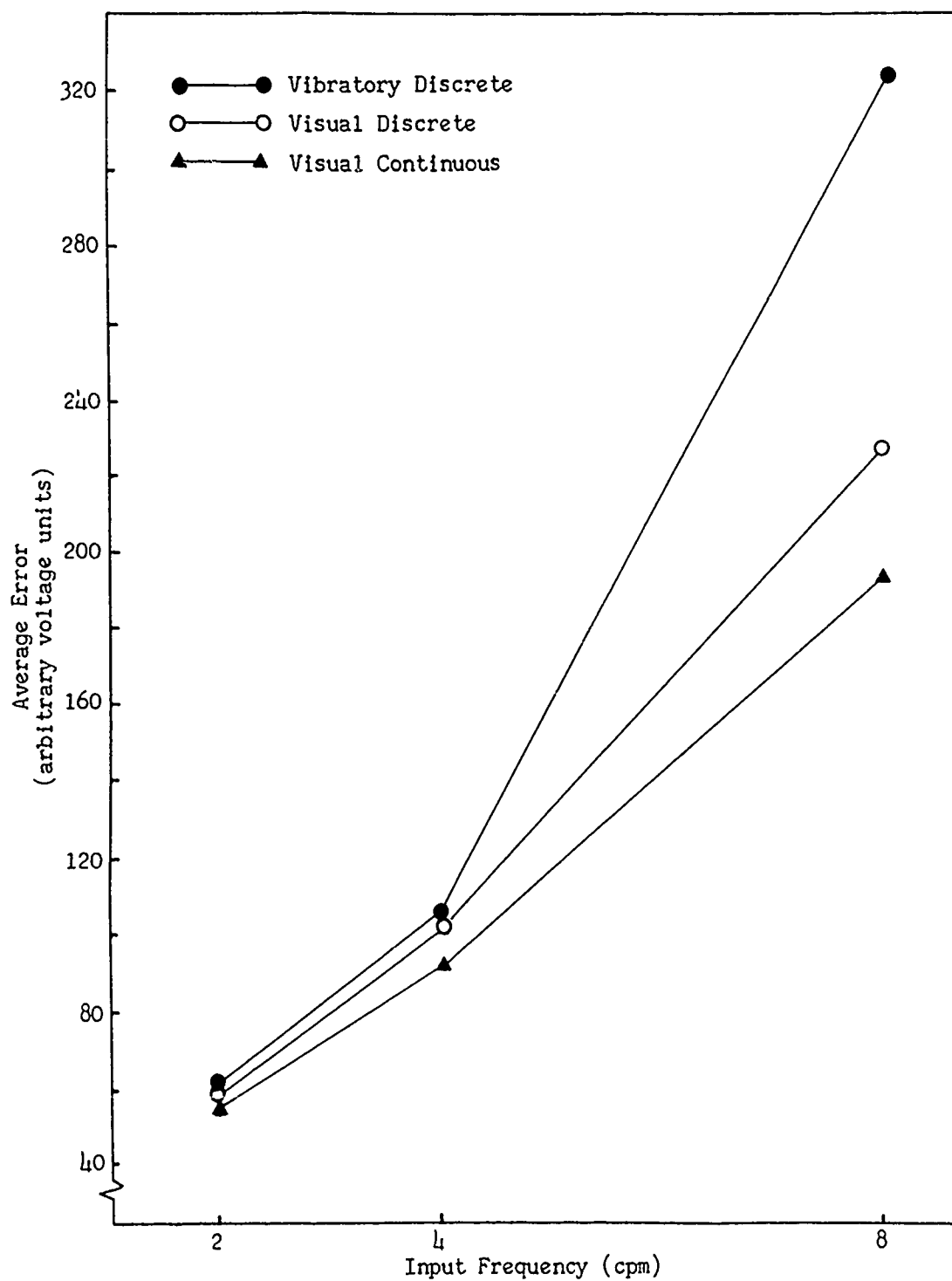


Fig. 2. Tracking error as a function of input frequency for three display modes in a quickened control system. (Data from Howell and Briggs, 10)

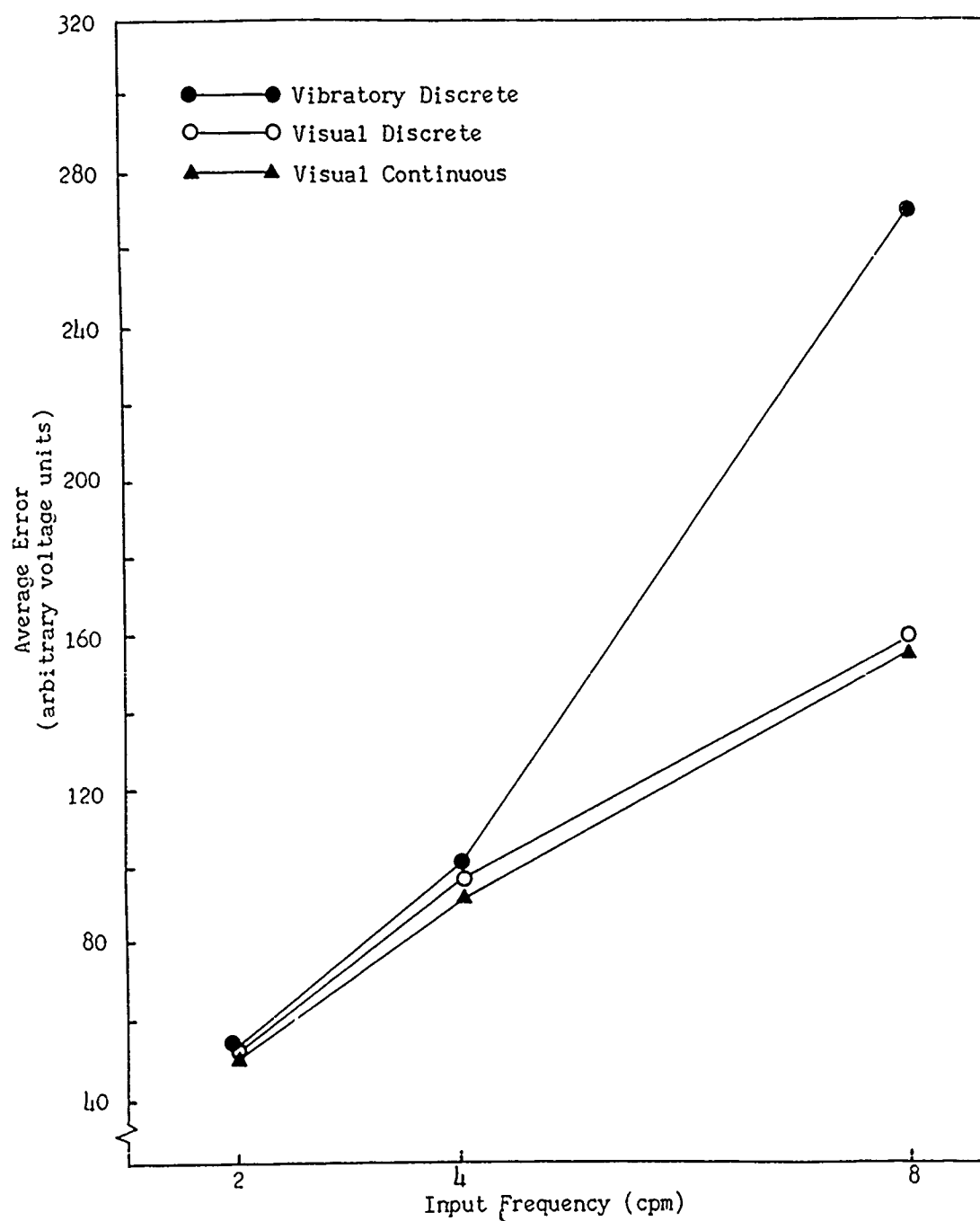


Fig. 3. Tracking error as a function of input frequency for three display modes in a super-quickened control system. (Data from Howell and Briggs, 10)

continuous visual display was negligible. That no such improvement occurred in the tactual mode at the higher frequencies may indicate only that the training period was insufficiently long. A bit of subjective evidence favoring this position was volunteered by both subjects: they observed that the response seemed less automatic (i. e., required more conscious effort) using the vibrotactile display than using the discrete visual display, although the perceptual differences among the elements appeared no less great. This report also suggests a second possible explanation: the compatibility relations involved in the vibrotactile tracking task may have been poor. This situation would certainly be most apparent at the higher frequencies. To the best of my knowledge, interest has only recently been kindled in the compatibility problem for vibratory tracking, so that the feasibility of this explanation is, at this writing, difficult to assess.

Another possible explanation is embodied in the old problem of instrumentation. Although the vibrators used in this study were equated for loudness on the chest and pressure was kept at a fairly constant level, it was still possible for the observer to reduce this balance by assuming unusual bodily positions. Such an unbalance would tend to cause a "loss" of the signal for a fraction of a second, a condition sufficient to build up a large error score. The size of the error built up, in fact, would increase as a function of frequency.

The picture, then, is far from clear regarding the practicability of a tactual display in the presentation of continuous signals. It does appear, however, that with more adequate instrumentation and proper training the tactual mode could be used in many of the tracking tasks for which vision is now being used exclusively. Why, for example, could not the operator use the two interchangeably, relying on the tactual mode for the more gross, routine aspects of control, and the visual mode for the occasional fine adjustments? This would free the visual channel during much of the operation for tasks to which it is more uniquely suited. It seems, in short, a waste of resources for an instrument with capabilities of the eye to be relegated to performing gross tracking tasks over long periods of time when other suitable channels are available. Here, I believe, is one of the areas in which the tactual display has a great contribution to make.

Another such area is that embodied in our last category of information: the category defined by the low frequency of signal occurrence. Here, again, I feel that the tactual display has a unique contribution to make, although my evidence in this case is more theoretical than

empirical. Let us begin our discussion by considering the peculiar problems posed by the infrequent input situation. In general, tasks involving infrequent signals place a high value upon these signals: i. e., when a signal does occur, it is important. On the other hand, evidence is plentiful to show that as events become more infrequent and as the human is required to observe for longer periods of time, he becomes much more apt to miss the signal (4, 5, 9, 12). This is apparently not altogether a perceptual problem, but an attentional one as well (5). Thus, we may distinguish two factors which appear to be involved in such tasks: an attentional factor which is a prerequisite for detecting anything, and a perceptual factor which determines the ability of an observer to identify a signal once he is aware of it.

With regard to the attentional aspect of the problem, I think that there is good evidence to suggest that the decrement in vigilance over time may be attributed, at a neurophysiological level, to insufficient stimulation of the subcortical arousal system (4). This center, the subject of much recent psychophysiological investigation, has been called the general activation mechanism, or more specifically, the ascending brain stem reticular formation (13). There is little cause for me to go into a broad discussion of this mechanism here, except to note that (a) it has been demonstrated to be a general regulator of the ability of other parts of the nervous system to receive incoming signals, and (b) it appears to be fed by fibres from all sensory systems, being responsive to over-all quantity rather than specificity of stimulation (13). In view of these characteristics, it follows that a reduction in over-all level of stimulation such as is apt to occur in a vigilance task, would reduce the responsiveness of the neural mechanism regulating sensory and perceptual experience and would thereby impair the capacity of the operator to detect important signals. In essence, then, I am attributing detection failure in monitoring tasks to neurophysiological events which appear to underlie 'attention' as it has typically been defined (4). The validity of this suggestion is, of course, capable of experimental tests, for both the neurophysiological and behavioral phenomena involved are amenable to independent, and even simultaneous, measurement.

I would like to suggest that tactual displays could be used in at least two ways to help overcome the monitoring problem, provided, of course, the neurophysiological explanation given above is a tenable one. First, and most obviously, they could be used to increase the over-all level of stimulation in the organism and thus maintain the state of arousal necessary for the detection of visual signals (7). Second, they might be used as a means of displaying relevant information per se,

in that tactual signals would accompany visual signals (and thereby serve as redundant cues) or that they would replace visual signals as the only source of relevant information. But what reason is there to suspect that the tactual system could be superior to the visual system in vigilance? Well, for one thing, the signals--as in audition--would be inescapable by the observer (7). More important, however, is the possibility that the tactual sense may contribute more heavily to the general activation level of the organism than the visual sense. Several investigators have found differences between sensory modes with respect to their ability to arouse cortical activity: nociception and proprioception offering the greatest contribution, audition and vision the least (1, 13). It appears, therefore, that in regard to the attentional aspect of the vigilance task the tactual system may be far superior to the conventional visual mode for displaying information.

My argument is lacking, as I mentioned earlier, in direct empirical evidence regarding the relative capabilities of the visual and tactual modes for displaying infrequent signals. This situation should not be allowed to continue. The need for improved displays to combat vigilance decrements increases daily with each step in our march toward automation. More and more the human is being required to serve as a monitor in control systems, even though, unfortunately, his ability to function as a monitor is no better than it ever was. The solution to this problem, it appears, must come in the form of improved displays.

We have touched upon a broad spectrum of communication problems in this brief exposition; not with the intent of providing solutions, but with the aim of pointing out the possible contributions which the tactual mode could make to each. In terms of the criteria of need, practicability of instrumentation, unique characteristics, and possible limitations, we have considered the feasibility of the tactual display in relation to several classes of input information: those characterized by frequent signals and those characterized by infrequent signals; those containing a high degree of uncertainty and those containing a low degree of uncertainty; those displayed via continuous signals and those displayed via discrete signals. We have concluded, on the basis of the best available evidence, that the most fertile areas for the development of tactual displays are those concerned with the presentation of frequent discrete (or even continuous) signals with a low degree of uncertainty, or those concerned with the display of extremely infrequent discrete signals. Recent developments in the Virginia (2) and the Fort Knox (8) laboratories indicate that better instruments and presentation techniques are now on the way for both mechanical and electrical modes of stimulation. Such

developments may, of course, render the foregoing recommendations obsolete! In any event, let us hope that such improvements lead the way to a much clearer understanding of the potential of tactual displays.

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On Optimizing Cutaneous Communication:
A Respectful Supplement to Some Adventures in Tactile Literacy

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Man is tactually literate! This has been known for some time--for at least as long as the blind have been known to "read" with their finger tips scanning Braille symbols. Helen Adams Keller not only reads with her fingers and writes with them through a typewriter, but also she "listens" with the skin of her hands and "speaks" with her fingers in a semiprivate system of encoding and decoding the kinds of information ordinarily called "linguistic" or "literate." Man's integument can be substituted in linguistic matters for his ears, his eyes, or for both.

Geldard (15) has referred to the work of both Gault (14) and Katz (20) who thirty years ago investigated the feasibility of circumventing deafness by applying speech sounds directly to the surface of the skin through stimulus-producing electromechanical transducers. Their results, and those of more recent investigations (32), suggest that this is not feasible. In spite of the many similarities between hearing and skin sensations--and there are many as Békésy (4) has shown--such attempts appear futile in the light of our present knowledge. It does not appear possible to make any simple direct transcriptions of speech-patterned sound energy that when applied to the skin result in meaningful percepts of speech patterns. The limiting features of the skin seem to be the slowness with which its nerves react relative to the auditory nerves, and the non-independence of stimulus frequency and intensity in the cutaneous sense (2, 4, 15).

A caution is necessary, however, that this conclusion not be over-generalized. There is at least one datum to indicate that speech sounds may be directly perceived through the skin, albeit the sensory transcriptions may not be simple and direct. Whether complex or simple, cutaneous transcriptions of such speech-patterned sound energies apparently are perceived and interpreted directly as "speech" in at least one instance!

Specifically, Helen Keller is able to interpret speech that she "hears" through whatever stimuli act on three fingers--one placed on the throat just over the larynx, one placed just in front of the lips, and the third

placed just beside the nose of the speaker. The resulting cutaneous sensations are apparently sufficiently strong and separate to have permitted her to develop some vocalization, or "speech," herself.

For the present, however, this one instance will be ignored and the previous conclusion will be accepted. When this is done, it follows that cutaneous communication can take place only through the symbolic encoding of language. That is to say, there will have to be some sort of mediation whereby a cutaneous stimulus ("signal" or "symbol" might be a more appropriate term) is interpreted as a symbol for some literate or linguistic form which, in turn, is taken to represent some element of language or speech. If this is to be, it might be well to review the available evidence concerning man's ability to handle such symbolically encoded information. Such a review will be attempted in this paper.

No attempt will be made to review the basic psychophysical, physiological, or psychophysiological data that describe the cutaneous sense, except insofar as these data define the symbol sets available for the construction of alternative stimulus-alphabets. More attention will be paid to those data that relate to man's information-handling abilities, particularly to his abilities in encoding and decoding information--or, put a little more operationally, that relate to the construction of optimal S-R ensembles. Finally, a few words will be devoted to those relatively unexplored areas relating response-alphabets and information-handling tasks; it will be hypothesized that whereas some response-alphabets may be optimally suited to decoding and transmitting activities, others may be better suited to the assimilation activities of an "informational destination."

Cutaneous Sensitivity and Symbol Sets

The exciting adventures in tactile literacy that have taken place at the University of Virginia over the past several years provide a stimulating point of departure (for a summary, see 15). The results of their psychophysical studies with mechanical vibratory stimuli have indicated that within practical limits, man is limited to two or three steps of intensity¹,

¹Hawkes (16, 17) has extended this work to the area of electrical stimulation of the skin. In addition to the construction of a scale of estimated magnitude of intensity as a function of the electrical stimulus, he has conducted investigations of the ability of man to make absolute identifications of such intensities. He concludes that, as with mechanical vibration, about two or three steps of intensity are all that can be used practically in constructing a cutaneous alphabet.

three or four steps of duration, and six or seven steps of position or locus on the chest region (15, 18, 29, 30). In addition, Howell (19) found that there are probably two or three steps possible in the discrimination of rates of change of intensities of vibration. It would be possible to increase the number of steps of position were it to be agreed to encompass a greater area of man's integument; this should be kept in mind, especially since the indications are that the "local sign" provides the most distinctive stimulus (15, p. 119).

These, then, constitute the basic stimulus sets from which an alphabet is to be constructed: two or three intensities, three or four durations, and a number of positions depending upon the area available. Numerosity (of simultaneous stimulation) might also provide another dimension, as might the combination of numerosity with position into "patterning," but data concerning these possibilities do not appear to be available.

The only possible way of obtaining a unidimensional alphabet of distinctive cutaneous symbols appears to depend upon the agreement to use a broader area of the skin. It might be that at least 26 distinctive "stimulus locations" could be found within the practical limits of usable skin area set by the distal goal of actually applying cutaneous communications in man-machine systems. If this is deemed either impossible or impractical, then the only alternative appears to be the use of combination symbols--or an alphabet based on multidimensional stimuli.

One such stimulus-alphabet has been used (15, 18). It was drawn from the set of 45 symbols possible through the combination of five locations with three durations and three intensities. Three subjects were able to learn to receive linguistic information (English words and short messages) with the resulting S-R ensemble, and one of these three was able to reach the remarkable point of handling 38 words per minute--a rate that exceeds the "expert" military standard for a receiver of Morse Code (24 words per minute).

With this dramatic demonstration, the question is no longer whether cutaneous communication is possible or even whether it is feasible. The question is, rather, "What are the upper limits of cutaneous communication?" or "What is man's channel capacity with this new medium of language?" Any attempt to answer this question definitively requires optimization of the conditions under which performance is to be measured (1). Probably the most important of these conditions, in terms of proportional contribution to the over-all performance variance, is set by the make-up of the specific S-R ensemble used. Thus, attention

must be directed specifically toward the construction of S-R ensembles; i. e., to the pairings of stimuli from the possible alphabets of cutaneous symbols with responses from alphabets that are also symbols, but symbols from one of the several possible response-alphabets of literate or linguistic forms.

Alphabets, S-R Ensembles, and Optimum Coding

Some research bearing on the optimization of S-R ensembles has been published. The available data, however, fall far short of providing a complete systematic guide to the construction of such optimum symbolic codes. Nevertheless, a few valid generalizations do appear possible, and it will be to illustrate these that the review presented in this section is intended.

Compatibility Effects

In order to have a usable code, it is not sufficient (although it may be quite necessary) to have a set of discriminable stimuli. These stimuli must be arranged into an alphabet of symbols. To this stimulus-alphabet there must be matched a response-alphabet, an alphabet of response symbols drawn from some set of discriminable responses. When this matching has been accomplished, a "code" or "S-R ensemble" has been constructed.

Man, the user of this code or S-R ensemble, must be able to learn to associate the response symbols with the stimulus symbols; he must learn to do this sufficiently well to make the correct response to each stimulus consistently. The relative ease with which (and the extent to which) these two goals can be approached has been found to be a function of the specific pairings of stimulus symbols and response symbols employed; the dependence of performance on this interaction has been called "S-R compatibility" (11).

An S-R compatibility effect is operationally defined as follows: m stimulus-alphabets are paired factorially with n response-alphabets to form mn different S-R ensembles. A different, but equivalent group of k subjects is then selected to work with each code. Any given subject is asked to decode the individual presentations of stimuli by responding in the manner appropriate to the specific S-R ensemble employed with his group. Performance is usually measured in terms of such things as reaction time, accuracy, information transmission, etc. The results of all kmn measurements are finally summarized in a manner

permitting the computation of an analysis of variance. If there is a significant interaction of stimulus-alphabets with response-alphabets, it is concluded that S-R compatibility effects have been demonstrated.

The effects of S-R compatibility are relatively large in magnitude (11), especially so in comparison with the effects either of short-term learning or of variations in the amount of information per stimulus (7, 10). Also, these effects are relatively stable, except under conditions of stress when the differences between ensembles of high and low compatibility may become accentuated. For example, it has been found that extraneous activity is likely to affect performance least with the most compatible ensembles and to interfere most with the least compatible codes (12).

In short, compatible S-R ensembles tend to be optimal coding schemes, and the optimum codes tend to be those consisting of highly compatible S-R ensembles. It follows that the study of compatibility effects (and the maximizing of compatibility) is basic to the construction of an optimal S-R ensemble for cutaneous communication.

Maximizing Compatibility

It has been observed that relative S-R compatibility tends to approach a maximum (a) when the stimulus and response alphabets correspond to one another in a direct physical sense, and (b) when the pairings of stimulus and response elements in the ensemble agree with strong individual and population stereotypes, where such stereotypes exist (see 7, pp. 319f). These may be taken as two sub-criteria for maximizing compatibility: correspondence and stereotypy.

Correspondence between alphabets.--The emphasis on physical correspondence between stimulus and response alphabets is probably an error! The initial generalization making this emphasis was based upon a series of S-R compatibility studies, all of which were concentrated on perceptual-motor performance. Most of these studies, especially the earlier ones, used ensembles formed of light-patterned stimuli on the one hand, and motor-patterned responses on the other.

For example, Fitts and his colleagues at the Ohio State University (7, 10, 11) generally required their subjects to move a stylus quickly in the direction indicated by an encoded visual stimulus. The studies conducted at the University of Wisconsin by Grant and his colleagues (3, 25, 27) required key-pressing responses to light stimuli; those conducted

at the US Naval Research Laboratory by Garvey and his co-workers (12, 13, 23) also used push-button responses to light stimuli and, in addition, used some auditory symbols (23). From the results of these studies, it is clear that relative S-R compatibility tends to be maximized in proportion to the degree of direct physical correspondence between stimulus and response alphabets; this conclusion appears valid for generalization to other ensembles employing similar types of perceptual-motor performances.

However, if the motor aspects of performance are not emphasized, then it is suggested that the conclusion is overlimited in its emphasis on the physical aspects of correspondence. It should be broadened to include conceptual, or at least "dimensional" aspects of correspondence. In fact, a recommendation has been made elsewhere (26, p. 90) to preserve the multidimensional aspects of combination stimuli by selecting multidimensional response-alphabets. This recommendation for achieving optimum coding was based principally on a study completed about five years ago.

In this study, Slivinske, Bennett, and Irby (26, pp. 74-83) compared the performance of subjects in learning and responding to 13 different symbol-readout combinations. Four stimulus-alphabets were used. Each was based on the absolute identification of a visual symbol; all symbols employed one or more lines set at different visual inclinations, so that "lineal inclination" was a primary dimension in each stimulus-alphabet. Other dimensions were also used, and all stimuli consisted of combination symbols.

In the "clock code" each symbol consisted of two lines (like the hands of a clock), but the long hand was restricted to the 3, 6, 9, and 12 o'clock positions and the short hand was restricted to the other clock-hour positions. The "diameter code" consisted of symbols each of which was a diameter extending on either side of a central hub; the diameters could take any of ten positions 18 deg. apart and could be any of three different lengths. The "radius code" was similar to the "diameter code" except that the single line in each symbol radiated from the central hub (rather than extending through it) and could take any of the 12 hour positions of the clock; the different inclinations were again combined with three possible line lengths. The fourth, and final stimulus-alphabet used consisted of symbols making up a "binary code" in which the different categories were formed by the presence or absence of one or more of eight radius lines. Thirty symbols were employed with each of these four stimulus-alphabets.

Three readout systems (or response-alphabets) were combined with each of the stimulus-alphabets. A "letter-number readout" was used; the symbols of this response-alphabet consisted of two parts, a letter ("A" through "D") and a number. A "number-number readout" was also used; this was similar to the "letter-number readout" except that both elements of the response symbol were numerals. Finally, a "numerical-progression readout" system was used in which the numerals "1" to "30" were serially assigned to the symbols of the stimulus-alphabets.

In addition to the 12 S-R ensembles constructed through the factorial pairing of the four stimulus-alphabets with the three response-alphabets, the "clock code" stimulus-alphabet was also paired with a "clock readout" response-alphabet.

Stimulus symbols were presented visually by means of a slide projector. Subjects--130 of them, ten randomly assigned to each of 13 groups, with each group assigned one and only one S-R ensemble--responded by writing on prepared answer sheets the readout symbol they associated with each presentation of a stimulus symbol. A total of 660 responses was obtained from each subject, or 85,800 responses in all.

The data were analyzed in terms of the mean number of correct responses. An analysis of variance was computed. This analysis indicated that not only were there significant differences among stimulus-alphabets and among response-alphabets ("radius code" was best, diameter code" was worst, and the "numerical-progression readout" was not used as accurately as the other readouts), but also there was a significant interaction indicative of a compatibility effect. The interpretation of the compatibility effect appears to indicate clearly that maximum compatibility is obtained when the readout preserves the multidimensional aspect of the stimulus--i. e., when the stimulus and response alphabets show "dimensional" correspondence.

To summarize this point, it may be said that the closer the conceptual of "dimensional" correspondence between the alphabets of cutaneous symbols and language symbols to be employed, the better the chance that compatibility will be maximized, and the more likely that the resulting S-R ensemble will be an optimal one. This may be of particular importance in cutaneous communication since it was noted earlier that alphabets of multidimensional cutaneous symbols are likely to be found necessary.

Population stereotypes. -- The determination of population stereotypes is essentially an empirical task. It involves the discovery among subjects of any preferences in the specific assignments of response symbols to stimulus symbols. Such preferences or stereotypes are found, if not universally then at least frequently. At times they are found to be contradictory to "standard" design practices (cf. 24). Whether they will be found in the case of specific sets of cutaneous stimuli and language symbols to be used in cutaneous communication will be known only after appropriate investigations have been conducted.

What can be said at present is this: If strong individual or population stereotypes are found, then the pairing of elements in the stimulus and response alphabets should agree with these stereotypes. Doing so should tend to maximize compatibility and the likelihood of constructing an optimal S-R ensemble.

Information-Handling and Response Sets

How is man to be used in this system of cutaneous communication? Is he to be used as a message decoder or information transmitter, or is he to be used as a message destination or assimilator of information? How well might the literate forms of English be expected to suit either usage? How well the linguistic forms?

Literate Forms

The 26 letters of the English alphabet have certain merits to recommend their use in any scheme for symbolizing language: First, the letters are familiar discriminable stimulus and response symbols. They have been learned (in the senses of recognition, reproduction, etc.) by all literate English-speaking people and by some people who speak other languages. One would not have to waste time teaching men these symbols for use in a response-alphabet. Secondly, they form probably the shortest response-alphabet that could be used--an alphabet consisting of only 26 elements. This may be an important advantage if it is decided to use a unidimensional cutaneous symbol such as "local gn" exclusively in the stimulus-alphabet. Finally, the use of the 26 letters provides a maximum of redundancy in the transmission of ordinary English and should, therefore, result in more error-free transmission than other more efficient response-alphabets, all other things

If man is to be used as a decoder, priority should be given the use of these literate forms in the response-alphabet. It is well established that man can function as a fairly efficient decoder of various auditory and visual signals that he transcribes into "letters of the written language" or symbols of the literate alphabet. He does this in "reading" Morse Code and Semaphore, and at least one subject has done as well with symbolic vibratory stimulation of the skin (15, 18).

But what if man is to be used not merely as a message decoder, or an information transmitter! Suppose he is to be used as a message destination, or an assimilator of information. That is to say, suppose it is decided that the desirable goal is to have man perceive and interpret the cutaneous symbols directly as "speech," just as Helen Keller does through the use of her three fingers.

Might it not be possible to select a response-alphabet that would facilitate such perceptions? It might, but unfortunately the 26 letters of the English alphabet are probably poorly suited for the immediate perception of language in any form save the written; this can be inferred from what is known of the learning of Morse Code.

The classic studies of Bryan and Harter (5, 6), as reported by Woodworth (33, pp. 159 and 165-167) and by Woodworth and Schlosberg (34, pp. 810f), show the difficulties men have in learning to build the higher units of language perceptions out of Morse symbols. In receiving Morse Code, learning typically proceeds to a long plateau during which letters are apparently directly perceived, but not words or phrases. The acceleration of the learning curve above this plateau is associated with the subject's introspective report that he is beginning to recognize word patterns directly. A second long plateau, or final level of performance, is then reached during which words are directly perceived and the subject reports that he is beginning to recognize phrase patterns directly.

The most striking thing about these studies is the extremely long training periods required between successive accelerations; i. e., the extremely long length of the intermediate level or plateau (about 10-12 weeks). The second-most striking thing is that the intermediate plateau is not found in the learning curves relating to man's sending of Morse Code--only to his reception of the symbols.

From these findings it might be predicted that when man is acting as a message destination (as an assimilator of information), the literate

forms of English will not make a compatible S-R ensemble with any stimulus-alphabet except one that combines elements into non-temporal patterns corresponding to "printed" words and phrases. This prediction is based on two things: First, the hypothesis advanced to account for S-R compatibility effects involves the number of recoding steps required between stimulus and response, with the most compatible S-R ensembles being those codes demanding the minimum number of steps (see 9, 11; 7, pp. 317-319). Secondly, the use of the 26 letters of the English alphabet in anything other than printed form requires such recoding because in ordinary reading the letters are not perceived directly. Vernon (31) has put it as follows:

"There are two factors of essential importance which must be taken into consideration in studying the perception of words. First, all words are primarily speech units; the word as written and read derives directly from the word as spoken, and its origin is always implicit--hence some traces of auditory and vocal processes always occur during reading. ...the rapidity of reading is such--several words can be read during a fixation pause of the eyes of 0.3 seconds--that no exact sensory impression of the full details of the letter shapes can be obtained. At most a general 'word outline' is perceived, and the symbolized meaning of the words obtained immediately, without any awareness in ordinary reading of intermediate stages...." (31, pp. 37f).

It appears, therefore, that something other than the 26 letters of the English literate alphabet will have to be used as a response-alphabet if a highly compatible ensemble is to be constructed for the perception of cutaneously transmitted "speech." What of the possible use of linguistic forms, rather than literate forms?

Linguistic Forms

Pronunciation may be described with an alphabet of linguistic forms; the "phonemes" constitute such an alphabet. In turn, the phonemes may be symbolized visually by "letters" of different shapes--these constitute what is usually called a phonetic alphabet. The two alphabets should be distinguished from each other. One is an auditory or vocal alphabet of phonemes, whereas the other is a visual or written alphabet of symbols representing these phonemes.

There is only one alphabet of phonemes for any language such as English; this is the alphabet of linguistic forms or basic sounds that

occur in speaking that language. However, there may be any number of phonetic alphabets to symbolize these forms. One alphabet of printed symbols has been standardized by the International Phonetic Association; this is the set of symbols commonly referred to as "the phonetic alphabet." It has been reproduced in many dictionaries along with the simpler symbols used in respelling for pronunciation (see 21); it is generally used alone in a pronouncing dictionary (e. g., 22).

The number of separate symbols in a phonetic alphabet usually falls between 50 and 70, depending upon the alphabet. The number of distinctive phonemes actually discriminable in speech may be fewer--say around 40 or so. Finally, it is likely that a response-alphabet of even fewer symbols or "sounds" could be used in cutaneous communication, since essentially no one person uses all the phonemes in a language.

A phonetic alphabet, or the phonemes themselves could be used as a response-alphabet; it might be that one of them could be paired with some cutaneous stimulus-alphabet to form a highly compatible ensemble. Further experimentation will tell. Two major disadvantages do appear, however. First, neither the phonemes nor phonetic alphabets are very familiar to many literate English-speaking people. The training time necessary for subjects to learn such response-alphabets would probably preclude the widespread use of any S-R ensembles that employed them; this is not to say that they might not prove quite effective for specialized use, even in cutaneous communication. They might! The second disadvantage is similar to the major objection raised to the use of literate forms--men do not ordinarily perceive individual phonemes when they hear speech anymore than they perceive individual letters when they read.

Combined Forms

The use of some combinational literate-linguistic form might offer a possible solution to this problem of selecting an appropriate response-alphabet. A set of ideal forms would have the familiarity of the "letters" to combine with the implicit perceptibility of "speech." It is suggested that the use of a syllabic alphabet for the responses might meet these criteria.

This suggestion could be put to test more-or-less immediately in any of several ways. For example, the semi-syllabic alphabets used in stenography or stenotypy might be tried. Better yet, if literate Japanese-speaking people could be obtained as subjects, a very

powerful test of the worthwhileness of this suggestion could be made. This would require the construction of an S-R ensemble for cutaneous communication in Japanese!

This is why. Japanese children learn to read and write first by studying 50 elementary kana symbols. These symbols (either Kata-kana, the "block" form, or Hira-gana, the "script" form) are used to represent a basic syllabic alphabet. The alphabet is bidimensional: one dimension is that of "vowelness" (a, i, u, e, and o), and the other is that of "consonantness" (no consonant, k, s, t, n, h, m, y, r, and w). Thus, symbols in the kana alphabet represent syllables such as a, ka, sa, ta, na, ha, ma, ya, ra, and wa. The addition of a third dimension is used to transform certain consonants to either a harder or a softer form; e. g., ka to ga, sa to za, ta to da, and ha to either ba or pa (cf. 28, p. A-5).

All literate Japanese speakers are well schooled in the use of these syllabic symbols. A reasonable stimulus-alphabet of cutaneous stimuli could be constructed to have "dimensional" correspondence with the kana alphabet. For example, the stimulus-alphabet could consist of five durations (for "vowelness"), ten locations (for "consonantness"), and two intensities (for "hardness" of consonants). The extent to which success is achieved in such a study, relative to the success already achieved with English and the letters of the alphabet (15, 18), should illustrate what advantages might be expected from further attempts to construct an optimal S-R ensemble for cutaneous communication in English.

A Postscript

Ten years ago, in 1950, a group of ten leading experimental and engineering psychologists met. They met to prepare a long-range integrated plan for human engineering research to parallel and support long-range planning for equipment and systems design. As a result of their efforts, a report was prepared for the Air Navigation Development Board under the auspices of the National Research Council's Committee on Aviation Psychology (8). Although intended for the development of an optimal air-navigation and traffic-control system, their research program had both depth and breadth; with a few relatively minor changes in wording it could be applied to the development of many modern complex man-machine systems.

The second of nine broad research objectives listed was the "determination of the capacities of human operators for handling information in a communication system" (8, pp. xiv and 18). Despite all the

research conducted during the past ten years it still seems like good advice to recommend the study of man's information-handling abilities and such things as "...the rate at which a human can assimilate information, and the conditions that permit a maximum rate...." (8, p. 17). Add to this a recommendation to study man's perception of language and linguistic information, and it probably still will seem like good advice ten years from now!

Summary

Man's ability to handle symbolically encoded linguistic information has been the general topic of this paper. Specific attention has been given to studies that bear directly on the transmission of linguistic information tactually, and on optimizing such cutaneous communication.

The available data indicate not only that such communication is possible, but also that it may be quite feasible and efficient. The basic symbol sets from which a practical stimulus-alphabet may be constructed (with mechanical vibratory stimulation of the skin) appear to be limited to two or three intensities, three or four durations, and a number of locations depending upon the skin area employed (e. g., six or seven on the chest). The use of other dimensions of cutaneous sensitivity and other methods of stimulating the skin may extend this symbol set.

Two generalizations, well founded on empirical evidence, were presented to show how optimum codes might be constructed. They refer principally to the construction of maximally compatible S-R ensembles through the use of "correspondence" and "stereotypy." The former demands that the elements of the stimulus and response alphabets correspond to one another in a direct physical sense or, at least, in a less direct conceptual or "dimensional" sense. The latter demands that the pairings of stimulus and response elements in the ensemble agree with strong individual and population stereotypes, where such stereotypes exist.

Finally, some suggestions--less well founded on directly applicable empirical evidence--were made. Most of these represent hypotheses concerning how specific response-alphabets (e. g., literate vs. linguistic forms) might be expected to interact with man's information-handling tasks (e. g., decoding vs. assimilating linguistic information) in a cutaneous communication system. It was specifically hypothesized that use of a syllabic alphabet might combine the advantageous

familiarity of the literate forms with the implicit perceptibility of the linguistic or speech forms. Such a combination of merits might produce a nearly "ideal" response-alphabet.

Whether this hypothesis will be supported by the results of an empirical test is, at present, "anybody's guess." The extent to which cutaneous communication can be optimized is also largely unknown at the present. There is one fact that is undeniable, as Geldard (15) has previously shown: Man is, or can be, tactually literate.

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The Unfinished Chapter

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The skin, as a sense organ, is rather like the retina, being capable of spatial discriminations. The skin is rather like the ear with regard to temporal discrimination. Consequently I thought it might be interesting to see what happens if one undertakes to outline a chapter on the cutaneous senses using headings borrowed from chapters on vision and hearing. Another reason for trying this is that there is probably not a whole new set of principles involved in each different sense department. Therefore, we might learn something by trying to discover which phenomena from vision or audition are not limited to those senses, but have their cutaneous counterparts.

I should say that I am not going to try and cover all possible topic headings today. In some cases I couldn't think of any very relevant analogy. For instance, I didn't come up with a good cutaneous analog of color constancy. Then, too, there really isn't time to do it all today. The other thing I should say before I begin is that the result of this enterprise turned out to be less a way of systematizing already existing knowledge than a way of pointing out where some research is needed.

A good place for a chapter on the senses to begin is with the nature of the stimulus. For the cutaneous senses, specification of the physical stimulus seems straightforward enough - physics had to solve this problem for its purposes a long time ago. However, if the aim is to specify which aspect of stimulation is important for sensation, the matter becomes less straightforward. For instance, though von Frey could specify that for touch, tension is the stimulus, not force, what about temperature sensations? Were Bazett, and Oppel and Hardy, (1937b), right in saying that temperature gradients within the skin were the important specification of the stimulus? Or do we believe, with Kitchell, Strom, and Zotterman, that temperature at the receptor is what counts?

For pain, trying to specify the stimulus really leads to chaos. It is not merely a matter of relating grams per square millimeter to gram-calories per second per square centimeter to milliamperes. What is

the common event by which various stimuli yield pain? The commonly expressed hypothesis is that it is tissue damage that counts - that an intensity of stimulation sufficient to damage tissue will be signalled by pain. This won't do. It would be convenient if we were always warned that tissue damage is beginning by the fact that we hurt, but more than one man has learned that this isn't so - pain is not always an accurate indicator of the onset of hearing damage, and the early X-ray workers would not have used the hand as a test object while adjusting the X-ray beam if the resulting damage had then and there been accompanied by pain. I suspect that the question will not be answered without some good quantitative microchemistry.

The next section heading in this chapter would be description of the receptor. For many authors, this section was easier to write a long time ago than it is now. One would describe the Ruffini cylinder, the Pacinian corpuscle, the Krause end bulb, and such other encapsulations as he had a mind to, as well as the free nerve ending. Then one would correlate the various sensory qualities with the different types of ending, with a level of confidence ranging from tentative to complete, depending on how little attention he paid to the evidence. He would probably find it convenient not to try to explain why the Krause end bulb would be a receptor especially sensitive to cold.

Then, as you know, Weddell and his co-workers began emphasizing how much of our skin is populated by nerve endings which make do without any encapsulations, and Loewenstein whittled off most of the Pacinian corpuscle without inconveniencing the receptor function of the remainder. Of course, the fact that I can get along without my glasses does not mean that I'm not helped by having them on, and the analogy may hold for the encapsulations. Still it now appears that the bare nerve ending is a very versatile thing. An iconoclast might well feel that the encapsulation represented merely a callous surrounding the nerve fiber which in some way or other had irritated the tissue. I suspect that before we can write anything very definitive in this section on description of the receptor, there will be involved some rather sophisticated biophysics, and that the electron microscope will have had a hand in unraveling the story.

Spectral sensitivity is a topic relevant to both vision and audition, thresholds and equal sensation curves being plotted against wavelength or frequency. Similar curves could be plotted for cutaneous warmth, with wavelength of infrared radiation as the independent variable. Indeed, Oppel and Hardy (1937b) did something along these lines for warmth, and the same could be done for thermal pain. I think, however, that in

this case we have an illustration of an analogy which doesn't help much, since such efforts are merely roundabout ways of estimating the internal temperatures resulting from radiant stimulation.

More interest attaches to the spectral sensitivity of touch, if one makes it possible for there to be a spectrum for touch by applying stimulation repetitively, as vibration. You all know where this story stands at present - absolute threshold curves showing maximum sensitivity between 200 - 300 cycles per second, and Sherrick's (1953) measurements showing the mechanical impedance of the skin to follow a similar function. We still need to know why pressure-sensitive spots show a flat frequency response, however. And in other ways, the story at the level of elementary psychophysics is by no means complete. For instance, almost any Elementary Psychology student can tell you what the upper frequency limit is for hearing. Nobody in this world can tell you what the upper frequency limit is for cutaneous vibration, (Geldard, p. 187). Of course, there have been technical problems in achieving the high energies at the high frequencies necessary to pursue this problem, but I should think that in this age of satellites the problem could be licked.

The fourth topic which occurs in any chapter on the senses is that of adaptation. Here I think there is some evidence that a phenomenon which has occurred in the studies of visual adaptation, for instance, is also found with the cutaneous senses. You are familiar with the fact that, if one measures the course of light adaptation by fatiguing one area of the retina, and then makes brightness matches with a comparison light falling on an unfatigued area, this method says that not more than three minutes is required for complete adaptation to occur. On the other hand, absolute threshold during light adaptation requires much, much longer to reach its new level.

The same thing may be true with regard to the cutaneous senses. For instance, Hawkes, using the instantaneous threshold method of measurement, found that adaptation to a level of alternating current twice threshold required from three to six minutes. Békésy (1959a) using the matching method and what I should judge to be not too dissimilar a level of adapting current, found adaptation to be complete in about a quarter of a minute. With the same method, he found that adaptation to mechanical vibration is much slower, the sensation level dropping about 10 decibels in 2 1/2 minutes. Comparison of the latter finding with the course of adaptation measured by the instantaneous threshold method fails because there are no good data so obtained. It

would be worthwhile to get them, if for no other reason than to discover how consistently the two methods, comparison stimulus and instantaneous threshold, yield different results. The two methods may be tapping very different processes.

When we talk about adaptation, we are talking about effects of stimulus intensity. Rather than taking up the conventional next topic, delta I, I'd like to go on, not to barely discriminable differences but to rather large intensity differences - in short, magnitude estimation. Stevens (1959) and Hawkes have both found that the exponent of the power function relating alternating current intensity to subjective magnitude is about 3.5 or more, and Hawkes has further shown that the exponent is unaffected by the frequency of the alternating current. Stevens (1959) has also reported that, for cutaneous mechanical vibration, the exponent is about 0.95. Gibson, in the Virginia laboratories, has extended the investigation to frequencies ranging from 30 c.p.s. to 650 c.p.s., and finds that the exponent for cutaneous mechanical vibration is also independent of frequency. I should mention here that Gibson's findings also support Stevens' suggestion that suprathreshold stimulus intensity, rather than total stimulus intensity, is the proper value to use.

Now there comes a puzzle. Given one equal-loudness function, and knowledge of the exponents resulting from magnitude estimation, it should be possible to generate all the equal-loudness functions for other intensities. So, with Gibson's findings at hand, I turned to some other available data to try this idea out. Preliminary to measurements of delta f for cutaneous mechanical vibration, Mrs. Goff obtained from her subjects two different equal-"loudness" contours, so that she could match stimuli of different frequencies for "loudness" before measuring delta f. Figure 1 shows you how with her data and Gibson's I attempted to predict one equal-loudness contour from the other. The curve marked "20 db." shows for the various frequencies the amplitudes which were judged equal to the standard 100-cycle stimulus at 20 db. above 100-cycle threshold. The point marked "35 db." shows the amplitude of a 25-cycle stimulus which the subject judged to equal in loudness that of the 100-cycle standard 35 db. above its threshold. Now, one statement of the power law is that equal stimulus ratios yield equal sensation ratios. For instance, if tripling a particular stimulus intensity doubles the magnitude of the corresponding sensation, then any value of stimulus intensity when tripled will yield double the corresponding sensation.

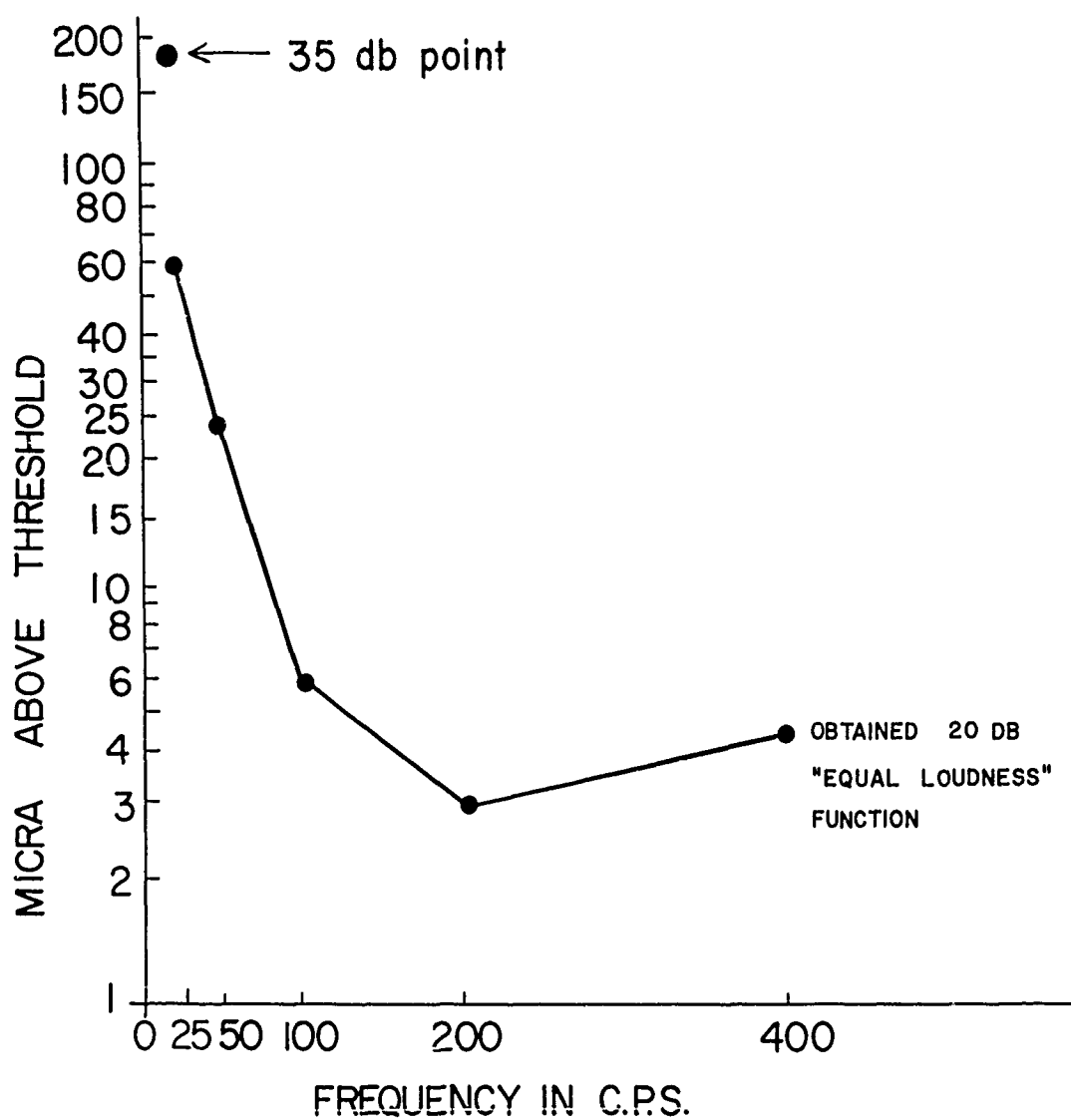


Fig. 1.

Since all values on the 20 db. line are of subjectively equal magnitude, and since equal stimulus ratios yield equal sensory ratios according to the power law, then if for each frequency we, say, triple the stimulus intensity which was judged equal to all the others on that contour, we should arrive at a new, higher contour which specifies another set of frequency-intensity combinations which should be judged equal to one another in subjective magnitude. It happens that in Figure 1 the 35 db. point is $3 \frac{1}{4}$ times the amplitude of the 20 db. point at that frequency. So, I simply multiplied all the 20 db. points expressed in micra above threshold, by $3 \frac{1}{4}$ to obtain the predicted 35 db. line you see in Figure 2. The result is shown by the dashed line, and the solid line shows the empirically obtained 35 db. line for this subject.

Clearly something didn't go right. One way of describing the discrepancy is that the power function exponent at the right side of the graph would need to be half as large as that at the left side of the graph in order for the predicted curve to coincide with the obtained one. Even making reasonable allowance for the inaccuracies of magnitude estimation and of equal-loudness matching, the discrepancy is still much too large.

I think there are three things I should point out here. First, of course, is that I have only shown you the results for one subject. However, the second point is that the treatment of the data I have just outlined for you does not assume any particular numerical value for the exponent of the power function, merely that for this individual it is the same value for all frequencies. Secondly, you are also correct in thinking that I have picked the case showing the most extreme discrepancy to show you first. To only a moderately lesser degree, however, this discrepancy also exists in the data from Mrs. Goff's other three subjects. The third point that should be made is that neither Gibson's nor Mrs. Goff's experiment was specifically directed to testing the possibility of obtaining equal loudness contours with the aid of magnitude estimation functions, and it would doubtless be desirable for someone to do so.

Turning now to other effects of stimulus intensity, there is the effect, well known in vision and in hearing, of intensity change of an otherwise invariant stimulus. Most hues shift as stimulus intensities vary, and stimulus intensity is also known to affect pitch. The same phenomenon occurs with either electrical or mechanical vibratory stimulation of the skin. Sherrick (1954), in his preliminary research on mechanical delta f , noticed that raising intensity lowered the perceived rate of vibration, and Békésy (1959a) has provided a valuable

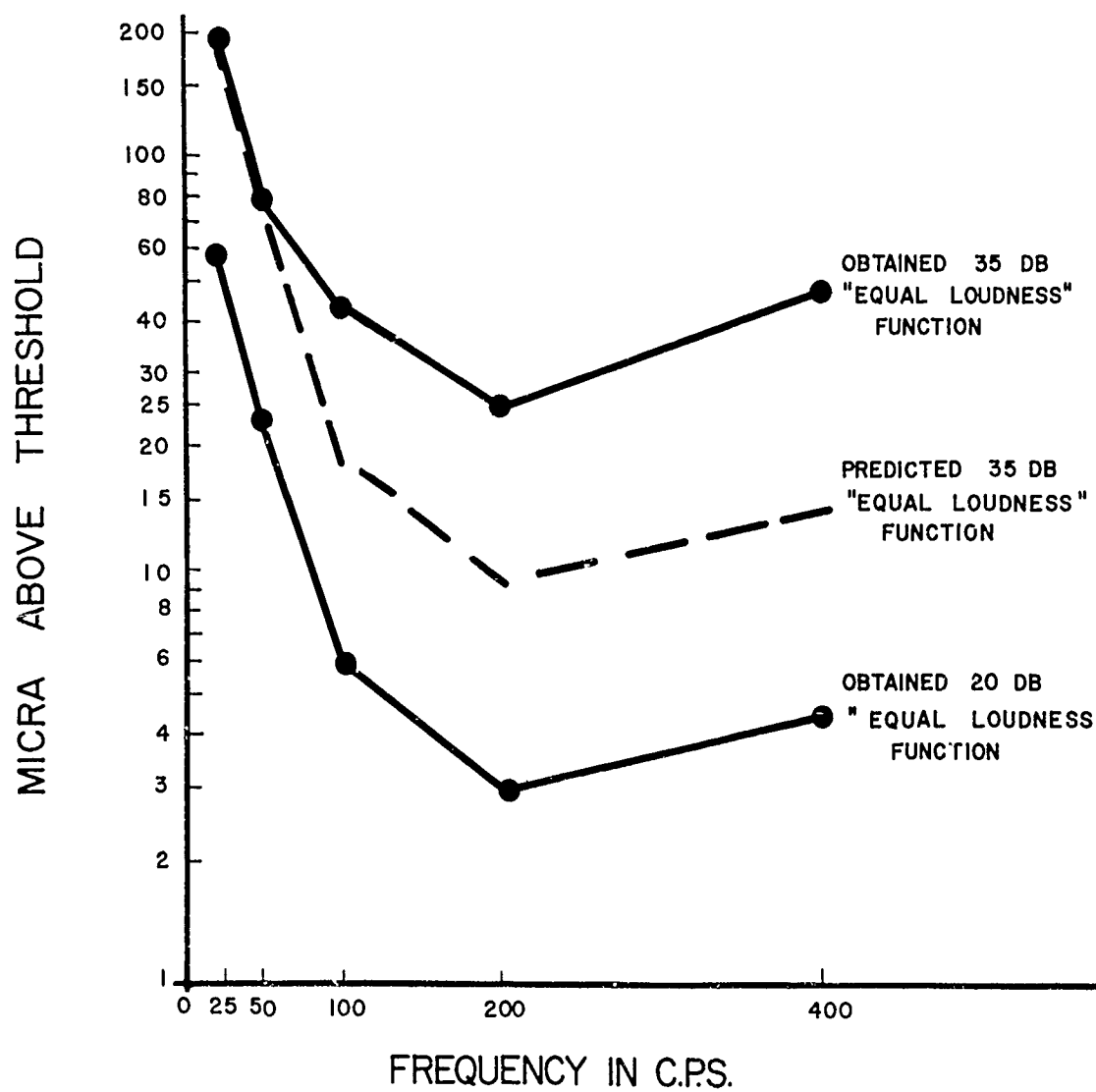


Fig. 2.

clue with the observation that the effect is influenced by changes in the area of the skin stimulated. It would be interesting to know how complete the analogy with the visual and auditory phenomena is. At low frequencies of cutaneous stimulation, an increased intensity leads to a lowering of pitch; are there invariant frequencies, at which intensity does not effect pitch? Are there frequencies at which increased intensity raises the pitch? How similar are vision and audition to the sense of touch in this regard?

We should not expect analogous phenomena to occur through all sense departments, of course. For instance, we know of a difference between vision and touch in the relative importance of factors concerned with apparent movement. Sumbly, in the Virginia laboratories, found that, when specifying the stimulus intensities necessary for the perception of vibratory apparent movement, all that was necessary was that the stimulus intensity be sufficient to be clearly felt. This is, of course, a quite different result than that in vision. The important factor in producing good cutaneous apparent movement turned out to be a one-tenth of a second difference between onsets of the two stimuli. Thus Korte's Laws do not hold unchanged for the sense of touch.

I think this was a good example of a useful and stimulating piece of research resulting from asking the question, "Does a certain visual spatial phenomenon have an analog in the spatial aspects of cutaneous sensation?" For a further source rich in examples of the value of taking questions originally conceived with regard to one sense and asking them with regard to the cutaneous sense, I would refer you to Békésy's recent series of reports on what he found when he asked auditory questions, and an occasional visual one, of the skin.

Before turning to examples of visual questions which can be asked of the skin, I cannot resist expressing surprise that we know so little about masking phenomena. Where are the Wegel and Lane graphs for cutaneous vibration? We can expect the results of a thorough investigation of masking to be more complicated than those found in audition because of the spatial characteristics of the skin. We can also expect the results to differ depending on whether, for instance, a signal on the forearm is masked by a signal at a nearby point on the arm, or whether the masking signal is applied to the seat. In view of both the theoretical and practical importance of knowing something about cutaneous masking, we can hope that these questions may not go too long unanswered.

There are many visual questions which might be asked of the skin. I will take time to mention just two. First, what is the metric of

cutaneous space? Is cutaneous space isotropic or anisotropic? The literature gives a pretty fair sampling of the resolving power of various parts of the skin, in terms of two-point threshold, but above the two-point threshold, are equal physical distances perceived as equal everywhere on the skin? Is five inches always five inches, everywhere and in every direction on the body surface? And, does cutaneous space behave like visual space with respect to the magic number 7? If there are about 7 absolutely identifiable distances in one dimension in visual space, are there also only about 7 absolutely identifiable loci in one dimension on the body? You and I would both doubt it, but where is the evidence? I think the value of finding out would lie not merely in adding one more item to George Miller's collection, but in giving us a little insight about what a dimension means in this context. Suppose there are 7 absolutely identifiable loci on the shin? If we add loci all around the leg are we in effect adding one dimension? Two dimensions? One and a half?

Finally, let me talk about negative after-images for a minute. I shall ask you, though, not to hold me to the analogy too precisely. I don't care to say what is the precise cutaneous analog of visually fixating a particular hue and immediately thereafter exposing the receptor surface to white light. Rather let me describe the procedure, for my purposes, as using one stimulus to cause adaptation, then seeing whether sensitivity to other stimuli is thereby changed. In olfaction, for example, adaptation to one odorous substance can change threshold not only for the substance but others as well. In the cutaneous senses, the shift of physiological zero with adaptation is perhaps the strongest single argument for common factors in the mechanisms involved in temperature sensations. These interrelations ought to be examined systematically for all the cutaneous qualities.

The fact that negative after-images are a case of successive contrast leads to the suggestion that we should investigate cutaneous simultaneous contrast. How does touch affect adjacent sensitivity to warmth, or vice versa? The nearest we come to such information is Weitz's finding that raising skin temperature by four degrees enhanced sensitivity to cutaneous mechanical vibration, and that greater warming of the skin then decreased sensitivity. One plausible explanation is of course the well-known temperature co-efficient of chemical reaction kinetics, followed by progressive denaturation of enzymes. There is also the possibility, though, that these results reflect the consequences of neural interaction, first summation, then interference. Just possibly, it might even be worthwhile to find out if the presence of mechanical

vibration effects sensitivity to warmth. There are a number of such conceivable interactions to be sought.

I have sketched for you, as I promised, some of the results of applying topic headings from chapters in vision and hearing to the cutaneous senses. Also as I promised you, it has turned out to be less a means of systematizing knowledge than to pointing to gaps in that knowledge. I have barely scratched the surface, but I hope you have been induced to try thinking in these terms, in the expectation of turning up some interesting questions that cry for empirical answers which would have theoretical implications.

I don't want to leave you with the impression that I am trying to promote a device for creative thinking, or a means of discovering research questions. My real concern is with the unity of the senses, which, in spite of their having each their own "tricks of the trade", have much in common with each other. Comparative study of the senses would seem to be a worthwhile pursuit, in the expectation that our understanding of them would thereby be materially increased.

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COMMENTS ON THE PRECEDING FIVE PAPERS

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I have been greatly interested in Dr. Gilmer's many-sided approach to the problem of getting closer to the receptor in defining the essential conditions of electrical stimulation, for this is what we should all like to achieve in all areas of sense. Mr. Gibson, in our laboratory, is attempting a semi-systematic exploration of this kind and is finding that the central problem is basically one of separating out pain or, at least, in determining whether it will ever be possible to deliver to the skin, any part of it, pain-free packages of electrical pulses suitable for coding. He is not, like Drs. Gregg and Alluisi, concerned with the coding problem itself. In general, there seems to be good agreement between Gilmer and Gibson as to what conditions must obtain to stay below the pain threshold. Uttal's experiments are in line, also. However, none suggests, so far, a range of potential variation in signal composition that holds much promise for a practical communication system. Pain seems always to be lurking around the corner when liberties are taken with any of the stimulus parameters.

It is good to see some exploratory work being done on electrodes, and Dr. Gilmer's use of tantalum and other inert materials may show some of our difficulties to be in electrode chemistry. At the same time, we have never supposed that any significant chemical interchanges were going on when ordinary precautions against them were taken. It would surprise me to learn that the source of pain was electroendosmotic. However, there are, of course, holes in the skin, and it may be that the salts of perspiration enter the electrolytic picture to complicate things.

Dr. Gregg, in his approach to the difficult problem of devising suitable coding systems, has the unenviable job of sloshing around in the apperceptive mass and coming up with something novel. Though he has obviously not progressed to the point in the development of a system where he is in a position to set up one that the rest of us can take cracks at, he is certainly proceeding in an orderly manner and is asking many of the right questions.

A model that permits a full and detailed analysis of the speech perception process would go a long way, as Dr. Gregg surmises, towards a workable cutaneous phoneme code. Others will tackle the problem of coding phonemes in a more direct, less systematic way. It will be interesting to see who wins the race.

The questions raised by Dr. Gregg's "Point 4"--What are the possibilities for simultaneous stimulation over dual cutaneous channels? Is it possible to put context-setting information into one cutaneous field, with the primary signal in another, splitting the total process between the two arms, e. g. ?--these suggest a variety of experiments of the Broadbent type. They ought to be done, for, whereas it will most certainly turn out that simultaneity of impression is not what is being achieved, a great deal of information would surely be transmitted rapidly with the central nervous system free to select what is needed, in some temporal order or other, from the two sensory sources.

Dr. Howell deserves our thanks for bringing up a most fundamental question, one that should never be allowed to escape our notice, viz., the essentially supplementary nature of the cutaneous channel in information-gathering. For crude signaling, touch alone will do; for fine signaling we must rely on the superior spatial characteristics found only in vision. Howell's tracking experiments are revealing here, showing, as they do, how poor performance can get when information is processed under relatively speeded conditions. He leaves some loopholes for interpretation: (a) more learning may have been needed, (b) compatibility of display was possibly poor, and (c) intensity of stimulation may have varied with body position. The first point (a), I shall not comment on; only experiment can decide. However, some things of relevance for (b) and (c) are even now happening. There is currently being carried out an experiment in which vibratory displays, arranged on the chest in radically different fashion, are being compared. Although the facts are not all in, it is beginning to appear that "compatibility" may not be such a critical matter in this sphere. At least, turning a display upside down doesn't make as much difference, apparently, as it ought to. On point (c), what is needed, of course, is some better engineering. I am happy to announce that that has just happened. Dr. R. C. Bice has, this past summer, come up with an inertia-type vibrator, constructed from a standard hearing-aid receiver, which is small and compact, difficult to damp, and which can be mounted on any desired place on the body surface. I have one with me and shall be glad to demonstrate the new receiver at the end of this session.

Another important matter alluded to by Dr. Howell concerns the place of cutaneous stimulation in raising the activation level of the organism by controlling or "steering" the reticular mechanism. Everyone is getting on this bandwagon these days, of course. At the M. I. T. symposium on sensory communication last summer the reticular activating substance got into the act with such frequency and had so many marvelous properties attributed to it as to have received the epithet, the "ridiculous aggravating substance." Others referred to it as the "reticular porridge." It may well be that ascending cutaneous paths have special potency, either in arousal or in maintaining vigilance. Some experiments performed by Dr. Gilmer a short time back suggest that attention to vibration does not readily flag.

With respect to Dr. Hahn's contribution, it would be presumptuous of me to attempt to say anything that would be news to him. Having engaged with him daily, over morning coffee, in speculations of the type his paper is made up of, I can only say that it is a rewarding thing, in sensory psychophysiology, to think through the phenomenal analogies. Indeed, his paper is proof of the point. I shall content myself with adding a few to his list. He knows about them but didn't want to appear absurd. I don't mind if I do. Here they are:

What would a cutaneous Munsell System be like? We had, way back there, Titchener's touch pyramid. What would such a construction look like today?

What is the cutaneous analog of the problem of "black?" Does the Gemeingefühl of Weber represent a steady state of adaptation, and is this the analog of G. E. Mueller's "brain gray?"

Can we devise a "standard cutaneous observer," like the standard color observer?

What, in somesthesia, corresponds to a chromaticity diagram, and what would it look like? Would it have the subsumptive power of its visual cousin?

What are the cutaneous analogs of Benham's top, Charpentier bands, and the Land color phenomena? There ought to be some. There are after-effects in the cutaneous sphere and we are also here dealing with an essentially spatial sense, as in vision.

Physiological nystagmus leads to some interesting and important visual problems. Especially is this true of the fading occurring in light

adaptation and the concomitant alterations in acuity. Is there a similar predicament in touch, with the vascular system moving the tissues constantly and with Rohrer's microvibration rippling over the skin surface constantly? Would immobilization lead to anesthesia? Anyone who repeats the Riggs experiment on the skin will have achieved something!

Flicker and flutter--what is the cutaneous analog? And this one brings us around full circle, for it was in an effort to answer this question, back in 1932, that Dr. Gilmer undertook his first work on vibration. You will agree it was a good question to ask--what is the c.f.f. for the skin?

Finally, I make my bow to Dr. Alluisi's thoughtful and provocative presentation. He does us the honor of picking up the "tactile literacy" problem and suggesting a way we might push forward. Two or three points should be singled out, I think:

1. "Numerosity," the present evidence indicates, is a false idol. Spector did the critical experiment several years ago. Several vibrators placed in the same general skin region, once static pressure has adapted out, feel no different from a single one. A time difference in onsets is necessary, if more than one localization is to result.

2. The distinctions outlined relative to compatibility, the principles of correspondence and stereotypy, are well taken; in constructing optimum codes we do well to get some guidance from this source.

3. The really promising suggestion is that concerning the 50 Japanese kana symbols. I should echo the comment of an earlier speaker--"I wish I had thought of that." As Dr. Alluisi says, this proposal should be carried out without too much difficulty. All of us have been held back by the thought that the investment in learning English phonemes would be a prodigious one, making the simplification and speeding up of the code a pretty difficult operation. But, apparently all that is needed is a subject who is a literate Jap or a subject who can learn what every Japanese schoolboy knows.

Observations Relating to Some Common Psychophysical
Functions as Applied to the Skin

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The subtitle of this paper should probably be "Incomplete Paragraphs in the Unfinished Chapter," since the experiments to be described represent attempts to answer questions raised by Dr. Hahn in his paper. The present writer did not rush back from Fort Knox and do these experiments; nevertheless, it was not felt that they were complete enough by themselves to qualify as proper material for a symposium talk until Dr. Hahn lent some organization to the writer's activities with his very lucid and stimulating discussion.

Three experimental problems that have been attacked since about 1958, and to which a part of the writer's time has been devoted since beginning work at the Central Institute for the Deaf in 1959, will be discussed. These may be titled as follows:

- (1) The frequency response of spots on the skin to vibrotactile forces.
- (2) a) The masking of tones by tones, both stimuli in one locus.
b) The masking of tones by tones and noise, masker in one locus, masked tone in another locus
- (3) Magnitude estimation of tones in quiet and noise in hearing and vibration.

I. The frequency response of spots on the skin to vibrotactile forces.

The history of this problem is long, but the papers having significance for the present study are those of Békésy (1939) and Geldard (1940). Békésy had indicated that the places on hairy skin of the arm most sensitive to pressure were displaced from those places that were maximally sensitive to vibration. Furthermore, he found that the frequency response of such vibratory "spots" was similar to that for the fingertip, but the minimum of the U-shaped curve was shifted toward the lower frequencies by one octave. Geldard, on the other hand (or rather, on the other side of the arm), found that vibratory sensitivity on hairless spots on the wrist was coincident with pressure sensitivity. The

variation of sensitivity with frequency was absent for these spots, between frequencies of 64 and 1024 cps.

This obvious discrepancy in results of two dependable research workers has lain unattended and uncared-for since that time, except for an unsuccessful attempt at repetition of Geldard's work by Jackson (1939), and a brief paragraph in the University of Virginia Research Planning Conference Notes titled "Repetition of Békésy's non-correspondence study." The present writer ignored the entire problem until 1957, when, apparently, sufficient time had elapsed that the mountainous problems of technique had been forgotten. At that time, pilot research was begun with two observers, and the slow process of accumulating threshold data from skin spots got under way.

The purpose of the investigation was twofold: First, to study the spatial relations of vibratory- and pressure-sensitive spots on both hairy and hairless skin; and second, to determine the frequency-response of spots on both kinds of skin. It was hypothesized that the presence of the hair in the skin might account for both the facts at issue, i. e., that vibratory spots are displaced from pressure spots, and that the frequency response on the two areas is different. The explanation might be, for example, that the hair represents a discontinuity in the medium propagating the energy, and hence may act to reflect waves so as to produce an apparent displacement of "receptor" position. Moreover, the hair by its reed-like behavior, might "tune in" certain frequencies of vibration to produce the dip in the sensitivity curve observed by Békésy.

The experiments were carried out as follows. Three normal male observers were trained to respond to the stimulation of spots on the skin by means of von Frey horsehairs of various pressures. Two areas were chosen for study. One, on the dorsal surface of the forearm, a hairy area, and the volar surface of the wrist, a bald area. A 400-square mm. area was marked off by a grid stamp, and with the aid of a 10x dissecting microscope, pressure sensitive spots were found by the usual psychophysical technique. The hair used produced a stimulus of 20.5 gm/mm². When a spot was found, its position was checked by carefully repeated stimulations to establish stability of position and point of maximal response. Immediately, thresholds for vibration were determined with a Goodmans V-47 vibrator to which was fastened a stiff hair with a diameter of 0.13 mm. The vibratory stimulus was provided by a Goodmans D-5 oscillator driving a Realistic high-fidelity 20-watt amplifier. The amplifier output was attenuated by a Centralab 4-ohm continuous attenuator, from which the signal went to the V-47 vibrator. A Ballantine

vacuum-tube voltmeter monitored the voltage across the vibrator coil, and from calibration curves the actual amplitude of vibration could be read.

Thresholds for vibration were obtained by a modified method of limits procedure. The E adjusted the intensity of vibration to a point well above or below threshold, and then tested for O's response by moving the vibrator (held in E's hands) until E could see through the microscope that the skin was just displaced. A small strong light on the microscope aided this manipulation. E then changed the intensity level in 1- or 2-decibel voltage steps and repeated the manipulation until O did or did not perceive the vibratory stimulus. Threshold was taken, for a series, as that point at which O first perceived or failed to perceive the stimulus. Four such series were taken for each frequency and each spot. Actually O perceived vibration against a 'noisy' background because of the steady pressure of the hair and the tremor of E's hand.

During the period when threshold determinations were made, E systematically moved the vibratory contactor over the area around the pressure spot to find the point of maximal sensitivity. This manipulation slowed down the entire procedure and was done carefully only for a set of four spots of each type. A map of the skin area was made showing landmarks (hairs, wrinkles, pores, etc.) and on this map the loci of pressure or vibratory spots were marked.

The qualitative results will be given first. Figure 1-A is a map of a typical hairy area studied, showing the location of a pressure spot and of vibratory spots. The vibratory spot is indeed displaced from the pressure spot, and there seem to be several spots that are equally good vibratory spots. Some are better at one frequency than are others. Whether the area is hairy or hairless makes relatively little difference, as Figure 1-B shows. The displacement is perhaps not so pronounced, and the number of alternative spots seems smaller. When E was getting thresholds, he used the information obtained to get the lowest threshold value possible, by stimulating all the loci that showed sensitivity before stopping a descending psychophysical series, and stopped an ascending series when the earliest response occurred.

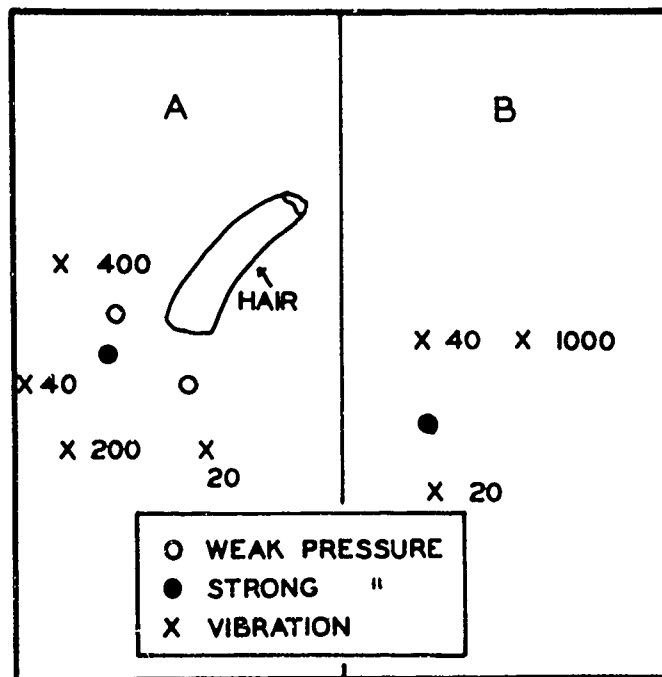


Fig. 1-A. A map of hairy skin showing the distribution of pressure-sensitive and vibration-sensitive spots. Numbers near spots indicate frequencies for which these spots were most sensitive.

Fig. 1-B. Same as A, but for hairless skin.

Thresholds were obtained from three Os on a total of ten hairy and ten bald spots, at frequencies of 20, 40, 100, 200, 400 and 1000 cps. Figure 2 shows the frequency-intensity function for these samples, in comparison with that obtained by Geldard.

I have no explanation for the discrepancy between these curves, but I do believe that a different hypothesis from those already mentioned is appropriate. Let us examine the work of Gilmer (1936) on the effect of contactor size, and of Keidel (1956) on the effect of pressure on absolute thresholds to vibration on the fingertip. Gilmer showed that a needle-point contactor shifted the frequency-response curve up to the 900-cps region from the 200-300 cps region. Keidel (1956, p. 90) showed that increases of pressure gradient, holding contactor size constant, produced a similar effect. It is my present belief that the threshold curves obtained with spots reflect this same effect, i. e., that the very great increase of pressure obtained by using small contactors serves to shift the minimum value for threshold to the higher frequencies.

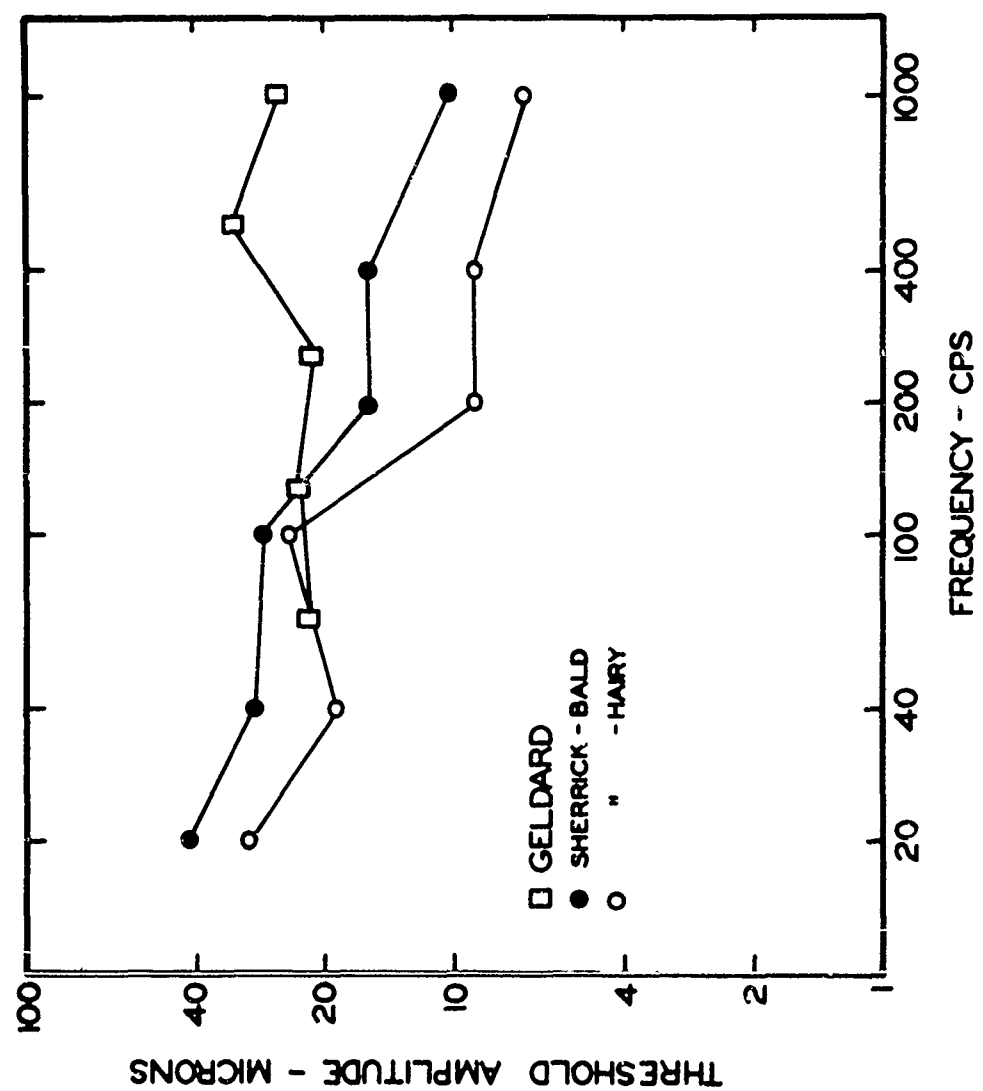


Fig. 2. The frequency-intensity function for hairy and hairless spots, compared with Geldard's (1940) function.

Another interesting effect is observed if one attempts to change slightly the area of the contactor. In a second experiment I did this, using contactors of 0.13, 0.26, and 0.46 mm diameter, on both hairy and hairless spots. Figure 3 shows the frequency-intensity function for this experiment, in which thresholds were obtained for five spots of each type from three Os, for frequencies of 20, 150, and 1000 cps. The function shows the characteristic mid-frequency dip with the middle-sized contactor, and this dip accentuates with the largest contactor. The odd function obtained previously reappears when the smallest contactor is used, however. Variation of point of maximal sensitivity with size is not demonstrable because only three frequencies were used.

I have temporarily reached an extinction level for this type of research, both manipulatively and verbally. The matter rests here.

II. The masking of tones by tones and noise.

1) The masking of tones by tones at one locus. One of the apparatus work-horses at the Central Institute for the Deaf is the Békésy Automatic Recording Audiometer (Békésy, 1947). By means of appropriate reconnections, one can drive a Goodmans V-47 vibrator from the same control rack. The frequency response of the vibrator is not like that of an earphone, however, so that some adjustment must be made of the threshold obtained from the record chart. An interest in the summer of 1959 in the problems of masking of vibratory patterns on the skin prompted the writer to do some pilot experiments with the Békésy audiometer, designed along the lines of Wegel and Lane's (1924) experiment. Absolute thresholds were measured in quiet and in the presence of masking tones of 60, 100, 200, and 400 cps, all at 30-db sensation level. The O held one finger on the vibrator contactor (1.0 cm. square) and with the other hand pressed the switch that moved the attenuator and recording element of the audiometer. A zig-zag tracing was thus drawn, indicating the decibels of signal attenuation O required for threshold at each frequency. Frequencies from 20 cps to 600 cps were studied, and Figure 4 shows the masked "vibrograms" obtained. The various masking frequencies appear to be ordered fairly well in the high-frequency end of the graph, i. e., the amount of masking is directly proportional to the frequency of the masker. Amount of masking was taken as the masked threshold in db re 1.0 micron minus the quiet threshold in db re 1.0 micron. This order is not preserved at the low frequencies, however. In fact, the amount of masking at the low frequencies is about the threshold shift one would expect from adaptation to a stimulus at a

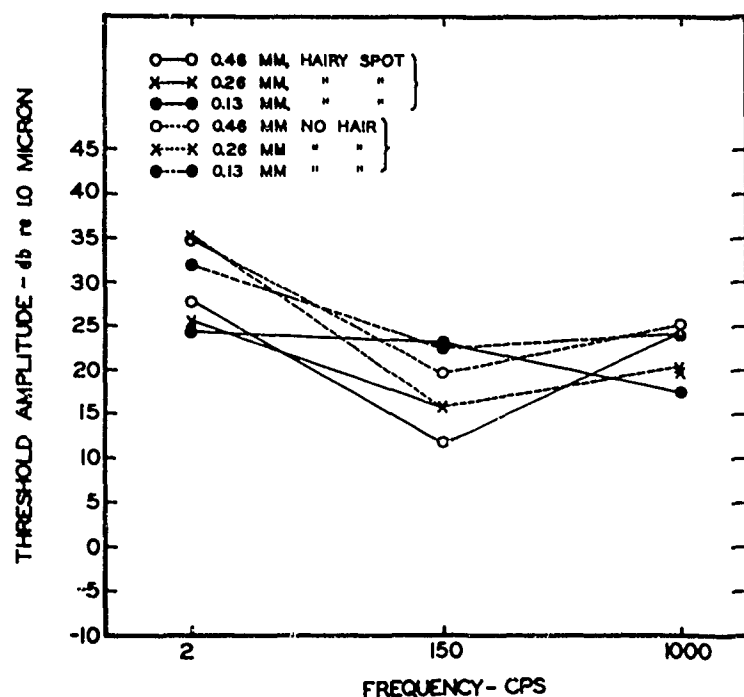


Fig. 3. The frequency-intensity functions obtained for various contactor sizes.

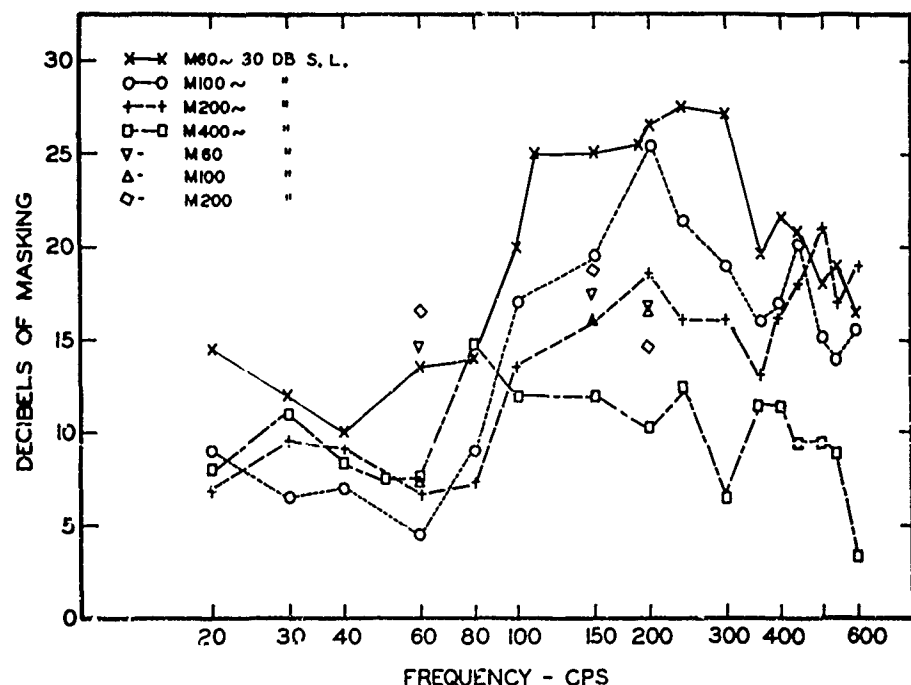


Fig. 4. Masking "vibrograms" for various frequencies of masking tones, all at 30 db sensation level. The isolated points unconnected by lines are for the two-locus conditions described in the text.

30-db sensation level. However since the effect of adaptation at one frequency on thresholds at other frequencies is not known, the data obtained are in some doubt. The writer feels now that discrete-frequency masking "vibrograms" must be taken to study the masking effect properly.

2) The masking of tones by tones and noise at two loci. Although it would seem that the present problem is simply a variant of the previous one, phenomenally the difference between the two is as great as that between brightness contrast and color contrast. Békésy (1959) has described the present situation several times, and in fact demonstrated the effect at the present symposium with the vibrating frame. A convincing demonstration can also be made in the following manner. Two vibrators are energized, one with a pure tone of say, 50 cps, the other with a random noise of the band from 20 - 100 cps. O is asked to touch the noise vibrator first with one hand, then to touch the vibrator energized by the tone with the other hand. He is asked to say if the vibrator touched second is active. He will say "no" or "not very strong." He then is told to lift his finger from the first vibrator. For all observers, the increase in intensity of the tone-energized vibrator is astounding. The effect is especially pronounced if the vibrators are switched on and off simultaneously without transients.

This effect appeared in some experiments being performed by Dr. I. J. Hirsh and the present writer on perception of temporal order. Observers were presented with mechanical pulses to the left and right index fingers independently, and were asked to tell which finger was stimulated first in time. E controlled the time delay between single pulses, and pulse intensity. Repetition rate was variable and was less than one pulse per second. When pulse intensities were adjusted for equal "loudness" by O, the temporal discrimination was as good as that of the ear (Hirsh, 1959). If intensities were different, however, O would often miss one of the pulses entirely (usually the weaker). This work is currently being pursued for other purposes. The neurologist's face-hand test is another example of this effect. When the patient is stimulated by touching, nearly simultaneously, the face and hand, he should report feeling both. If neurological damage is present, he may report only one locus stimulated (usually the face). This is called "extinction" by the neurologist.

All such effects seem to have a common basis in phenomena that have been elucidated by Békésy (1959). Small loudness imbalances at skin loci with sparse neural overlapping produce sudden shifts of sensation toward the side with the more intense stimulus. The study

of "masking" of this type, then implies the study of apparent shift of the locus stimulated. This work is still in its very early stages in the writer's laboratory.

The results of a preliminary experiment may be seen in Figure 4. In this experiment pure tones of 60, 100, or 200 cps were presented through a Goodmans V-47 vibrator to the palm of the hand at a distance of 14.0 cm. from the first Goodmans vibrator on the index fingertip. The fingertip stimulus was called a "masked" stimulus, the palmar stimulus the "masker". Absolute and masked thresholds were obtained for five Os at frequencies of 60, 100, and 200 cps, and masking level was always 30 db SL, re: fingertip threshold. The points not connected by lines show the amount of masking for the various frequencies. The curves are different from those obtained by the previous technique not only because two loci are involved, but also because in the second study, masker and masked stimulus were switched on and off simultaneously. Thus, O had to perceive a change in the spatial pattern of vibration, not in the intensive pattern. This is more analogous, however, to the situation in hearing. Os uniformly reported the appearance of a "phantom" vibration when the masked stimulus was raised in intensity above threshold, and if level increased further Os noted that the fingertip vibration began to "mask out" the palm vibration. The experience appealed to several Os with an interest in auditory localization problems.

This study is being pursued further to determine the systematic effects of varying such parameters as intensity level of masker, distance, phase relations between stimuli of the same frequency, etc. In addition, the studies begun in Dr. Hirsh's laboratory will be pursued.

III. Magnitude estimation of the loudness of tones in noise in hearing and vibration.

The present writer conducted a series of auditory experiments designed to determine the form of the magnitude estimation function when the 1000-cps tone was presented binaurally at various signal-to-noise ratios. The results of this experiment verified to a first approximation Stevens' (1959) hypothesis that the proper equation for magnitude estimation of loudness is

$$\psi = \kappa(\phi \cdot \phi_0)^{0.3}$$

where ψ is the loudness number assigned, κ a constant, ϕ is the physical value for the signal estimated, and ϕ_0 is the value of the signal threshold. The equation holds when noise is present if one takes as ϕ_0 the masked threshold of the tone. If the same experiment is repeated with vibratory stimuli, using eight levels of a 50-cps tone and a noise in the 20 - 100-cps band, both stimuli at one locus, the fingertip, the results are quite different. The 34-db tone was called 10. In this situation, the O adds the noise to the tone, and overestimates the loudness of the signal in the presence of the noise, as shown in Figure 5. He cannot discriminate tone from noise, of course, just as he could not in the threshold masking situation. The next step is to see what happens when the tone lies at one locus and the noise at a second locus. This is like the two-locus masking problem previously described, and perhaps more like the situation that exists in hearing. The observer is not likely to add noise and tone intensities together, but instead will perceive the tone as less intense as the signal-to-noise ratio falls. Work on this problem is presently under way, and results of a preliminary study are shown in Figure 5 in comparison with the single-locus condition. The same Os judged the magnitude of four vibration levels of a 50-cps tone, in "quiet" and in 34 db (re 1.0 micron R. M. S. amplitude) of a random noise band of 20 - 100 cps. In this experiment, as in the second masking experiment, the noise was presented through a Goodmans V-47 vibrator to the palm of the hand 14.0 cm. from the first vibrator at the index fingertip. The 38-db level of the 50-cps tone in quiet was called 10. Here we see the curve for the noise conditions rising toward the straight "quiet" loudness-value function just as it does in hearing.

The slopes of the "quiet" vibration magnitude curves are different mainly because of the small number of observations made. The distributions of the points obtained under the two types of noise condition do not overlap, however.

The present writer is not as yet prepared to state precisely the quantitative interrelations that exist, but from the research described, and from a careful reading of the publications of Dr. von Békésy, one would conclude that masking, loudness, and localization of transient and steady-state vibrotactile stimuli on the skin are all interrelated phenomena that are only superficially equated with intensity, temporal separation, and skin locus. The trading relations among these variables is plain to observe but difficult to quantify readily. The plan of the present writer is to pursue more intensively the neurologist's "extinction phenomenon" for the time being, and to return to the

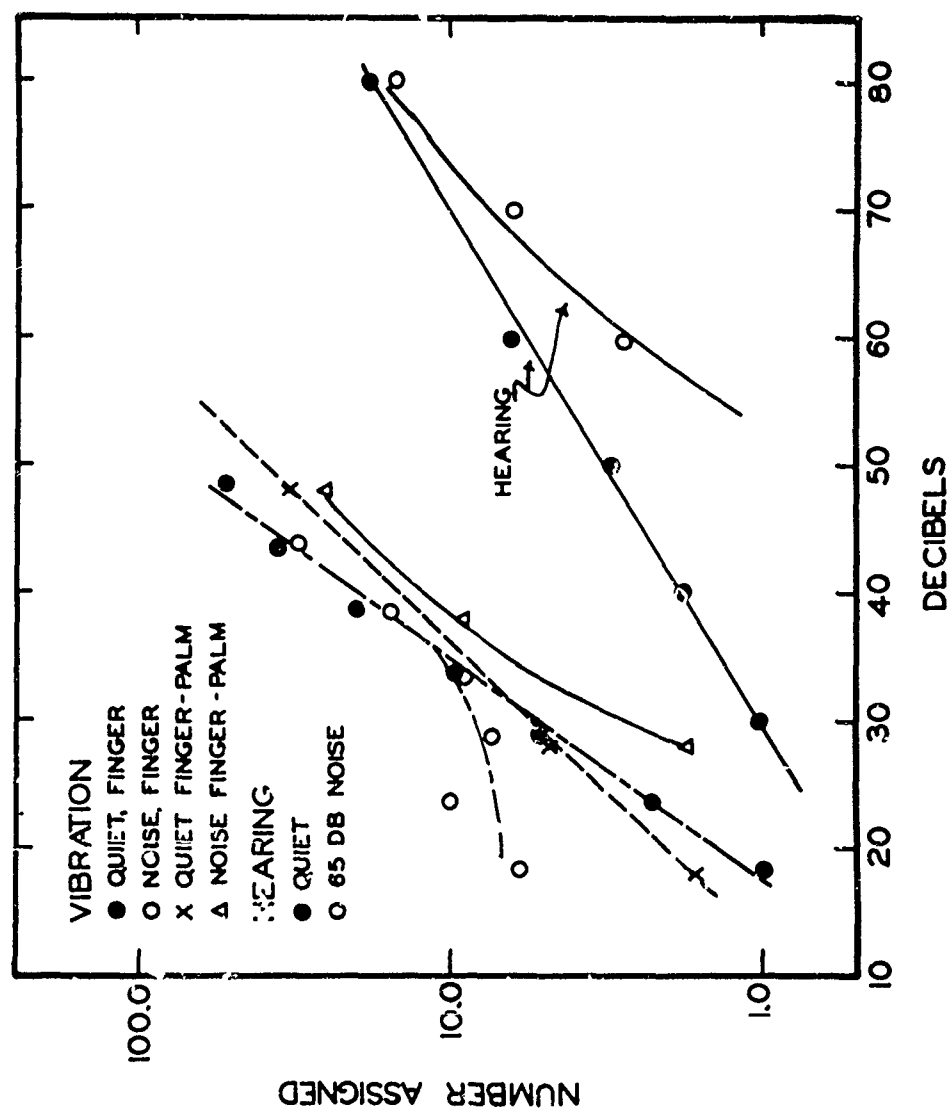


Fig. 5. Magnitude estimation of a 50-cps tone in quiet and in noise of the 20 to 100 cps band for stimulation at one and two loci. The analogous curve for hearing is shown for comparison purposes. For vibration, levels are db re 1.0 micron R.M.S.; for hearing, levels are db re 0.0002 microbar.

problems of masking, loudness, and adaptation of continuous tones or bands of noise, at a later date.

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CUTANEOUS RESEARCH POSSIBILITIES

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Although the title of this report might lead one to expect coverage of all aspects of cutaneous sensitivity, most of the papers represent research with use of mechanical vibration or electrical stimulation of the skin. These stimuli have been favored by the present investigators inasmuch as reasonably good control of the stimulus source is possible.

Much of the cutaneous research of past years has been devoted to the problem of what neural structure responds to stimulation of the skin. That this continues to be an important area of interest is attested to by the fact that several of the present papers are on this topic. The present group considers that Geldard's (1940) monograph has settled the matter of what neural mechanism is normally acted upon by mechanical cutaneous vibration. The touch mechanisms are affected, and vibration may be described simply as "pressure in motion." The problem remains, however, of determining the exact characteristics of these touch mechanisms.

The question of identifying the neural structure whose response determines a sensation of warmth or cold is a matter of considerable controversy, although some of the present group have definite ideas on what mechanisms respond to temperature changes (see Nafe's paper). The writer certainly agrees with Nafe and Kenshalo that the dependence of touch sensations on the action of Meissner corpuscles, of cold on Krause end-bulbs, etc., as suggested by von Frey, cannot be maintained today in the face of contradictory histological and electrophysiological data. Nothing in the evidence offered thus far, however, precludes the possibility that specialized neural terminations, where they do exist (e. g., the fingertip and forearm), make a contribution to sensation.

The most salient problem of cutaneous research in the present writer's opinion is the need for a determination of the cutaneous discriminations possible with use of various stimuli. Good information

¹For reviews of cutaneous research, the reader's attention is invited to Geldard (1940), Keidel (1956), and von Békésy (1960).

does not exist at present on the dimensions of cutaneous discriminations; we must face the problem of ascertaining what sensations exist. The manipulations of a given stimulus dimension for a scaling of "pain" intensity, for example, are not necessarily comparable between various laboratories. In some studies, pain was reported to have "stretching" or "burning" characteristics, while at Fort Knox the interest has been in the appearance of a sensation analogous to that felt when a needle just penetrates the skin. These problems become even more acute when we consider complex perceptions such as "oiliness," "clamminess," and a host of others. This writer feels that the introspective technique might well prove to be invaluable in resolving such difficulties.

Nearly every conceivable problem in the area of cutaneous research needs additional work. The writer feels, however, that the problem of determining the characteristics of the transducing mechanism whereby applied energy serves to elicit nerve impulses is of particular importance at this time. The following list of more specific research problems might stimulate interest in elucidating the transduction process:

The effect of skin temperature changes on sensitivity to mechanical vibration. Warming or cooling the skin affects the RL for mechanical vibration, indicating that the touch mechanisms may operate via chemical reactions, i. e., via a chemical transducer. Information is needed on the RL and DL with localized vs. generalized heating or cooling of the body such as is obtained in fever conditions, or drug-induced changes.

Interaction among the various senses. This may be considered to be directly related to the above. In the above situation, both warmth and vibratory sensations may simultaneously exist and remain clearly distinct. With some combinations of sensations, however, qualitative changes occur, as with the subjective fusion of cold and pressure to produce a sensation of "wetness." How and why such qualitative changes occur should be studied.

Adaptation and accommodation. Present data on adaptation to mechanical cutaneous vibration indicate little more than that a vibrating stick held by the hands results in a feeling of "pressury numbness" after an hour or so of continuous stimulation. Further, accommodation apparently does not occur under some circumstances. Almost nothing is known of the course of the adaptation-recovery cycle.

Locus variations in sensitivity. Certain body areas appear to be relatively insensitive or analgesic, e. g., Kiesow's area. Little data are available to indicate the possible presence of other such areas, or

of the existence of areas of particularly high or low sensitivity to such stimuli as thermal changes. Systematic explorations of the body surface are needed to determine what sensitivity variations exist, and to relate them to such things as, e.g., density of innervation.

Temporal discrimination. Information is needed on discrimination of the minimal interval, of temporal order, and even on the minimum duration of stimulation necessary to elicit sensations.

The above list and the preceding discussion has made little mention of an inadequate stimulus, electrical current, which under appropriate conditions will elicit any of the sensations discussed. The application of electrical current to the skin may result in sensations of warmth, cold, pressure, pain, or tingle. Of these, however, only tingle and pain may routinely be elicited. It is a vexing problem for our understanding of what cutaneous discriminations are possible that direct current will elicit tingling sensations analogous to those produced by alternating current. More specifically, what properties of our nervous system result in a discontinuous sensation both from the application of a continuous stimulus (direct current) and a discontinuous one (alternating current). In addition, alternating current applied to the skin cannot be discriminated from mechanical cutaneous vibration in some instances. Identification of the independent dimensions of cutaneous discriminations could help to elucidate such problems.

The writer is of the opinion that electrical current may represent an unique stimulus source in that it first normally stimulates the nerves directly, as well as receptors (Hawkes, 1960b). It is recognized that sufficiently intense stimulation of any sort may cause any of the cutaneous nerves to act, as in the firing of nerves maximally sensitive to mechanical pressure when sufficient cold is applied to the skin's surface.² Nevertheless, the intensity ratios necessary to elicit various kinds of sensations with electrical current are much smaller than those required with other stimuli. It is widely accepted that no nerves exist which are specialized for maximal response to electrical energy applied to the skin, yet there is a possibility that if we remain within an absolute energy concept all of the cutaneous nerves may be maximally sensitive to this kind of stimulation. It may be possible, therefore, to identify and elucidate the characteristics of the cutaneous neural mechanisms with electrical stimuli, comparing the data obtained thereby with psychophysical and electrophysiological data accumulated with other kinds of stimuli.

²A careful study by Hensel (1960) has indicated that certain cutaneous nerves respond to both mechanical and thermal stimulation, while some nerves respond only to pressure stimuli.

The problem areas delineated previously all need investigation with use of electrical cutaneous stimulation. In addition, the following problems represent a sample of research particularly in need of study if electrical stimuli are to be used to determine neural characteristics:

Frequency changes. Changing the frequency of alternating current affects the amount of current necessary to reach the RL; nothing is known of the effect of frequency of stimulation on quality of sensation, or, for that matter, on the DL for frequency.

Important stimulus parameters. The present group of investigators is in general agreement that the important parameter for intensity of sensation is current density at the receptor(s). In order to identify these parameters, much additional work is needed along the lines of Hahn's (1958) study.

Distribution or path of electrical current in the tissues. About all that is known at present of the action of electrical energy on the skin is that most of the resistance to the passage of current into the skin is due to the stratum corneum. Further, the skin has some capacitative and inductive properties, but nothing is known of the path of the energy through most of the epidermis or the cutis vera. Data on this problem might help to identify the neural mechanisms by indicating why, for example, more energy is usually necessary to elicit warmth sensations than tingle.

Arousal of various kinds of sensations. What manipulations of electrical cutaneous stimulation result in change of sensation quality from, e. g., cold to warmth? It is also conceivable that electrical current may be capable of eliciting sensations not aroused by other stimuli. Studies of sensitivity variations with locus may help to answer such questions.

Basic questions such as those listed above have considerable bearing on another main topic of interest to the present group of writers. Several of the papers of this report represent a relatively recent interest in the use of the skin as a receiver of information, i. e., its use as a communications channel. This interest was stimulated by Geldard's (1957) article on tactile literacy. The need for an additional communications channel exists for the blind or blind-deaf, in high-noise environments, on the battlefield (especially in the event of use of thermonuclear weapons) to give directional and other information, and for warning or alerting purposes.

A partial listing of research problems in cutaneous communications follows, again in the hope of stimulating research interest:

Kind of stimulation. The writer has suggested³ that electrical current is more useful than mechanical vibration for cutaneous signaling purposes. Comparisons of such communications systems have not been done.

Pain. The use of stimuli sufficiently intense so as to elicit pain may result in emotional disturbances. Well-trained subjects are not disturbed by such stimuli; nevertheless, the best use of painful stimuli for naive subjects may be for warning or alerting purposes. The sensory dynamic range for electrical stimulation of the skin is extremely small, and that for mechanical cutaneous vibration is somewhat larger but is probably considerable smaller than comparable ranges for audition and vision. Additional data are needed if the useful stimulus intensity ranges are to be specified for signals of very high or low frequencies of alternating current, for example.

Sensory differences for mechanical vibratory vs. electrical stimulation of the skin. Almost nothing is known on this problem.

Coding. Several of the present papers are devoted to this topic. The most promising approach to the problem of achieving speed of reception might be to use some sort of phoneme system (see particularly Alluisi's paper).

Evaluation criteria. Under what circumstances will cutaneous stimuli be of value? Are cutaneous stimuli as useful for signaling purposes as more conventional auditory or visual stimuli? Possible criteria are:

Training time. Geldard's subjects were able to receive messages at a rate of 38 words per minute, with a training time measured in days. This rate is faster than Morse Code reception for most receivers; Morse Code, however, is grossly inefficient compared to the reception of speech. What training times will be necessary if a phoneme system is used in order to speed up message reception rate? A first approach might be to try Japanese subjects, who are already trained in using the kana system.

Vigilance performance. This must be studied prior to any use of cutaneous stimuli for warning or alerting purposes. Needed are the

³A review of the writer's research on cutaneous communications has been presented elsewhere (Hawkes, 1960a).

assessment of vigilance performance with mechanical and electrical stimuli, a comparison to auditory and visual performance, and the use of more than one kind of stimulation in a given situation. The writer is now investigating these problems.

Use of cutaneous stimuli in the presence of other kinds of stimuli.
Will the addition of cutaneous stimuli to already present auditory or visual stimuli have a disrupting effect, or will it enable the observer to more closely approach his "information-handling capacity?" This has not been studied with use of cutaneous stimuli.

The value of the conference reported herein is based not only on the papers published in this report, but also on its effect upon the participants, resultant from informal group discussions and conclusions formed as an outgrowth of interaction between and among those present. At least two research programs in progress are attempting to answer questions posed above and in the preceding papers. Sherrick's paper represents a partial answer to some of the problems listed by Hahn; the writer is now studying vigilance performance with cutaneous and auditory stimuli as suggested by Gilmer and by Howell. The possibility of another meeting of the group within the next two years may furnish additional impetus to research effort.

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